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Observations on the Yellow Fever. *By* JOHN STEVENS, *Esq. of Hoboken, New Jersey.*

(Continued from page 10.)

THE contagious or non-contagious (or, rather, the infectious or non-infectious) nature of pestilential diseases has ever been a subject of controversy among physicians and philosophers. This diversity of opinion has arisen, not merely from a want of accurate observations, but very much from a vague and indefinite use of terms. Had the terms in use among medical men in their disputes on this subject been precisely and accurately defined, the controversy would probably have been determined long ago.

The term contagion has scarcely been used by any two authors in the same sense. The moderns, however, have given a latitude to the term which is not warranted either by its etymology or by the authority of the ancients. This term, in conformity to its derivation, was formerly restricted in its application to those diseases only which are communicable by immediate contact, such as itch, syphilis, leprosy, hydrophobia, &c. This limitation of the term, to preserve precision and and propriety in our discussions, should be again restored;

and to all diseases communicated by or through the atmosphere, the term infection should be applied.

Again, all infectious diseases arrange themselves under subdivisions resulting from characteristic distinctions in the nature of the diseases themselves. Thus, to those diseases which are communicated by the effluvia of secreted matter emanating from a diseased person, and without undergoing any change in passing through the atmosphere, the term specific infection ought to be exclusively appropriated. As those diseases which are to be ranked under this division are, with very few exceptions, not communicable a second time from one person to another, the arrangement is in strict conformity to nature.

Again, where a disease is contracted from some noxious quality in the air itself, it may, with strict propriety, be termed atmospheric infection.

Let me here indulge myself in a remark evincive of the beauty of this natural arrangement. Those diseases which are the product of contact merely, such as itch, leprosy, syphilis, &c. are in general loathsome, and seem to be designed by an allwise Providence as correctives against moral and physical depravity.

To diseases which are specifically infectious, many of them being extremely mortal, mankind are not in general liable to a second attack, as otherwise the world might be totally depopulated. Again, diseases originating from noxious qualities in the atmosphere itself, are for the most part, by due vigilance and attention, under the controul of man.

The arrangement here proposed obviates completely all difficulties and uncertainty respecting contagious and infectious diseases. If a disease is communicable by contact only, it is evidently a contagious disease. If it is communicable through the air, it is an infectious disease. If, at all seasons and in all situations, it is communicable immediately from one person to another, through the air, it is specifically infectious, especially if not to be taken a second time. There are, however, some specifically infectious diseases, which are also communicable by contact; for instance, small-pox. These diseases, then, are so far both contagious and infectious. But if, for the communication of a disease through the air, some noxious quality in that air is necessary, it then becomes an atmospheric infection. How easy is it, then, to determine in most cases whether a disease is specifically or atmospherically infectious. Let us take an instance of each species: small-pox and yellow fever. As the small-pox is communicable, through the air, from one person to another, at all seasons, and in all situations, and no person liable to a second attack of it, it is, unquestionably, a specific infection; but, on the other hand, as yellow fever is the effect of some noxious quality in the atmosphere itself, prevails only at certain seasons and in certain places, and as a person is liable to repeated attacks of it, can we hesitate a moment to pronounce it an atmospheric infection?

Atmospheric infections require the concurrence of certain seasons to render them epidemic. Universal experience has confirmed this fact. Thus, in Egypt, the plague commences in the latter part of the winter, and ceases with so much regularity that the natives are apprehensive of no danger after the 17th day of June. In Syria, also, its visitations are governed by similar periodical regularity. I need scarcely observe that

with us yellow fever has observed a like uniformity with respect to its appearance and disappearance.

This circumstance furnishes a characteristic distinction between specific infections and atmospheric infections. In a disease of the former description, the process on which its infecting quality depends is wholly completed in the body of the diseased person. Now, as it is well known, that the state of the fluids of the body, as well as its temperature, continue nearly the same at all seasons of the year, so we find that specific infections have scarcely any dependence on the vicissitudes of seasons and climates; whereas, on the other hand, in all atmospheric infections, the process on which their infecting quality depends is carried on in the atmosphere itself. Accordingly, therefore, to their respective natures, they require particular temperatures and seasons to bring them to maturity.

The ordinary process of putrefaction, whether of animal or vegetable substances, will not produce atmospheric infections. There is an inexplicable something, besides mere putrefaction, necessary to give a morbid quality to the atmosphere. The putrefaction of a dead horse, for instance, will annoy a whole neighbourhood with its stench, but generally no other inconvenience or mischief attends it. Thus, too, the farmer is under no apprehensions of danger from the putrefaction of animal and vegetable substances in his dung-heap, perhaps at his very door.

Dr. Mitchill* mentions a very remarkable fact corroborative of this doctrine. "Seven men," he observes, "belonging to the alms-house of New York, were employed, during the whole of the sickly season of 1798, in putting the persons dead of the plague (yellow fever) into coffins; and though they handled,

* Medical Repository, Vol. II, page 313.

in the course of their service, upwards of five hundred corpses, in different stages of putrefaction, and though they were much incommoded with the pestilential quality of the air in the rooms they entered, and frequently were obliged to vomit, not one of them was so much indisposed, during the whole of the season, as to discontinue his employment."

The celebrated Howard, speaking of the infectious nature of the plague, says, that "it is remarkable that, when the corpse is cold of a person dead of the plague, it does not infect the air by any noxious exhalations. This is so much believed in Turkey, that the people there are not afraid to handle such corpses. The governor of the French hospital at Smyrna told me, that in the last dreadful plague there, his house was rendered almost intolerable by an offensive scent; especially if he opened any of those windows which looked towards the great burying-ground, where numbers every day were left unburied; but that it had no effect on the health of himself or family."

These, and a thousand other facts of daily occurrence, prove, incontestibly, that the ordinary products of putrefactive processes are not alone the causes of atmospheric infections, but that some further process is required, which, of course, varies according to the nature of the disease thereby communicated.

Dr. Rush, in attributing yellow fever, in 1793, to putrid coffee, and Dr. Mitchill, in 1798, to putrid beef, were certainly incorrect; for, if those causes alone were sufficient to produce yellow fever, the disease would manifest itself every season, invariably, in various parts of the country: but this never happens. Out of sea-port towns yellow fever is nowhere to be found.

Some of the supporters of the domestic origin of the yellow fever, who have taken great pains too to prove that yellow fever is actually generated in the atmosphere of our cities, have, however, very inconsistently urged, that when yellow fever rages with peculiar malignancy, and becomes epidemic, that it becomes also specifically infectious. But when it is ascertained beyond all doubt that the atmosphere of a place will give the disease without any communication with the diseased, what evidence can we possibly have of any case of specific infection? If then, from the nature of the case, it is utterly impossible to adduce proof of specific infection in any one instance, it is surely inconsistent with reason and sound philosophy to have recourse to it, when every case that can possibly happen may be satisfactorily accounted for on the principles of atmospheric infection; and when we know, too, that the former has no existence whatever out of the sphere of the latter. The chain of reasoning here adduced propels us irresistibly to this conclusion: that yellow fever is never the result of specific infection, but is invariably the product of noxious combinations taking place in the atmosphere.

A cloud of evidence might, indeed, be adduced to prove that yellow fever is not specifically infectious. The exemption of physicians, nurses, washerwomen, and attendants, even in hospitals, besides the vast number of cases of individuals sickening and dying in the country without communicating infection, has placed this fact beyond the reach of controversy.

It will unquestionably be found, on investigation, that no malignant fever which evidently originates from noxious combinations taking place in the atmosphere itself, and therefore becomes epidemic, is, in any circumstance, the offspring of specific infection. Even the plague, which we have hitherto

been taught to believe so specifically infectious, is now proved to be not in the least so. Colonel Wilson, who has lately published a history of the British expedition in Egypt, and who, attached to the army, was himself on the spot, tells us expressly, "that the English and Turkish armies which marched to Cairo, passed through a country where the plague filled almost every village; they communicated, without any precaution, in the most intimate manner with the natives, established their ovens at Menouf, where the plague raged violently: the Turks even riffled the diseased in the pest-houses at Rhamanich, and at Cairo dug up the corpses recently buried, and yet no individual instance occurred of the malady in the armies."

In proof that jail fever also is in nowise specifically infectious, I will adduce two very remarkable cases.

First, The memorable catastrophe which happened at what are called the black assizes at Oxford, in the reign of queen Elizabeth. Camden's account of this affair is as follows. "While the judges of assizes sat at Oxford, and one Rowland Jenkes, a saucy foul-mouthed bookseller, was indicted for scandalous words against his princess, the greatest part of those who were there present, whether through a poisonous and pestilent vapour, or the stink of the prisoners, or damp of the ground, were taken in such a manner, that they died almost every one of them, within forty days, or thereabouts, except the women and children; and *none else were touched with the contagion.* Amongst those that died were Robert Bell, lord-chief baron of the exchequer, Sir Robert D'Oiley, and Sir William Babington, knights, D'Oiley, sheriff of Oxfordshire, Harcourt, Wineman, Fettiplace, men of great note in those parts; Barham, an excellent lawyer, almost all the jury (as they call 'em), and others to the number of 300, or

thereabouts." Stowe also tells us, that of this fever "died in Oxford 300 persons; and sickened there, but died elsewhere, more than 200 from the 6th to the 12th of July. After which died not one of that sickness, *for one of them infected not another.*"

The other is a similar case, which I here give in the words of Mr. Webster. "So also at the Old Bailey, not many years past, a few prisoners entering the court from a dirty jail, without changing their dress, infected a large number of persons who died; but no epidemic followed: the infection was soon dissipated, and there was an end of the disease."

These cases do not furnish ground even for a surmise that any disease was communicated from the prisoners themselves. And in neither case was the disease communicated, in a single instance, from those who caught it in court, to any other person.

The disease, in these cases, originated altogether from the noxious atmosphere with which the bodies of those prisoners were enveloped, and in nowise from individual infection: it is with the utmost precision and propriety, then, that the term atmosperic infection is applied to it.

That jail fever is not specifically infectious, the exemption of the nurses and attendants at the Marine Hospital affords ample testimony. Notwithstanding the vast numbers of emigrants who have, from time to time, been received into the wards of this hospital, yet have these nurses and attendants generally escaped sickness. Were this disease, indeed, individually infectious, in crowded ships and prisons, there would be

scarcely a possibility of escaping when the disease was prevalent.

That this disease, therefore, like yellow fever, plague, &c. is the result of noxious combinations formed in the atmosphere, and never from secreted matter emanating from a diseased person, is sufficiently evinced from the circumstances of the two cases above cited; but it differs from yellow fever, &c. in these respects: that it prevails chiefly in the winter season, and that the atmosphere in which it is generated is circumscribed within narrow bounds. I am inclined to think that the plagues which from time to time have formerly infested Europe during the winter season, have been of a nature similar to jail fever. The disease has lately raged in some of the manufacturing towns in England, with a degree of virulence and mortality not much inferior, within its sphere of action, to that of yellow fever in this country, during its worst stages.

As the gaseous compounds, whence atmospheric infections derive their origin, are capable of infinite diversification, so we may rationally presume, that the diseases they give rise to must also assume various types, analogous to the nature of these compounds. As plague, yellow fever, jail fever, &c. are always generated in cities or in crowded places, it is presumable that animal exhalations are the predominant ingredients of the combinations from whence they originate; and, on the other hand, that bilious fever, remitting fever, and intermitting fevers result principally from combinations framed from vegetable exhalations. Dysentery appears to be occasionally the result of either animal or vegetable exhalations, but assumes more or less malignancy in proportion to the predominancy of the animal ingredients. In camps, the infection is principally derived from excrementitious matters. Of all this numerous

family of atmospherically infectious diseases, none has been considered so specifically infectious as dysentery. But as no disease is more evidently the product of animal and vegetable exhalations, as it appears only in certain seasons and in certain places, and attacks the same person repeatedly, it is presumable we may, upon analogical principles, safely pronounce it at no time specifically infectious.

Should my theory prove to be well founded, the circumstances attending the propagation of dysentery will throw considerable light on the manner in which yellow fever also is propagated. It is an undoubted fact, that the excretions and excrementitious matters from a person labouring under this disease become highly infectious if not carefully removed. For this reason it is, that dysentery rages with such violence within an encampment; so that, in a short time, the atmosphere becomes extremely infectious.

In this particular the analogy between dysentery and yellow fever is very striking; for, in the latter disease, the remarkable case of Mrs. Baker, which will be mentioned presently, furnishes unequivocal proof of excretory matter in like manner becoming infectious; and there cannot, I think, be a shadow of doubt, but that, in favourable circumstances, a ferment of this nature would, in this disease, as in dysentery, infect a certain portion of the atmosphere. The infection of the plague is, beyond all doubt, conveyed from place to place by similar ferments; and the two instances already cited of prisoners in health communicating jail fever to numbers in the court, are direct and positive proofs that infection in this disease is also capable of being conveyed from place to place by *clothing;* and they are also striking instances in proof of this disease not being in any degree specifically infectious.

Should the theory I have advanced respecting diseases atmospherically infectious prove, on further investigation and experience, to be well founded, it will point out to us precisely where danger is to be apprehended; we shall avoid being taken by surprise, and being panic-stricken by false alarms. When we shall become thoroughly satisfied that a disease generated in the atmosphere can never, in any case, be communicated immediately from one person to another, but that it is only from excretory matter that danger is to be apprehended, we shall be taught where to guard against danger, and to perform our duties towards the sick with confidence and alacrity.

(To be continued.)

The Doctrine of Pulmonary Absorption defended. Communicated to the Editor by JOSEPH KLAPP, *M. D.*

IT is somewhat surprising that persons should be found to disbelieve absorption by the lungs, as it is so generally known that these organs are abundantly supplied with absorbing vessels, arising with large and patulous mouths from the air cells. If spirit of turpentine, mercury, and many other substances, be conveyed into the general circulation by the absorbents of the stomach and intestines, why may they not also be taken in when applied as fairly to the mouths of the lymphatics in the lungs, as can be the case in those organs, or any other? The existence of cuticular aborbents I must be permitted to regard as a mere hypothesis; and, being imaginary, no man can be authorized to demand a description of *their use*. But such

vessels have often been demonstrated to exist in the lungs, and if pulmonary absorption is to be denied, " as it is clear so large a congeries of vessels could never be proposed for no use, I think *we are warranted* in demanding what purpose they are intended to answer?"

In order to show that the lungs are abundantly supplied with absorbing vessels, I shall preface my experiments to prove that they do perform the office of absorption, by a few quotations from Mr. Cruikshank's work on the lymphatic system.

" Next to the liver, the lungs are that part in which I have found the greatest number of absorbent vessels." Vide page 175.

" The absorbents of the lungs are also divided into two sets, a superficial and deep seated. The superficial set, like those of the other viscera, are not always to be found. I have been able, at one time, to cover the whole external surface of the lungs with the absorbents I had injected; and at another time I have not been able to find one. The same thing I have known happen frequently in the liver.

" One of the easiest methods of finding them, is to inflate the lungs of a still-born child from the trachea; *the air passes instantly from the air cells*, and fills particularly the superficial absorbents; if a puncture is then made with a lancet into any one of those absorbents, the air will escape, and quicksilver may then be injected in its place, as the valves hinder it from escaping into the air cells. One of the most perfect preparations I ever made of the absorbents of the lungs was in this way." Vide page 176.

The advocates of non-cuticular absorption never intended to be understood that they believed the lungs "the exclusive apparatus of absorption." I always supposed they denied absorption from the surface only, and did not intend that their experiments and arguments to prove pulmonic absorption should be deemed by any one as an attempt to disprove its occurrence in the stomach, intestines, and in other parts. At least I can say, with regard to myself, such has always been my intention or meaning. Surely there could be no inconsistency in denying absorption from the surface, and at the same time *even contending* that it is executed by the pulmonary organs, as well as by all those parts in which it has been fully proved to exist*.

It is possible that, in stating the discovery of pulmonary absorption, its supporters may have spoken of it in rather an emphatic manner; but I am certain they thereby never thought of denouncing absorption from many other parts of the animal body. I trust the candid reader, and the most of writers, will unite in asserting, when the following experiments and other considerations are with deference submitted for their deliberation, that absorption by the lungs is not quite so vague, or hypothetical, as some have represented it. In the first place I shall take the liberty of quoting an experiment from my inaugural essay, to show that it is not quite true, as has been stated, that "in *every case* when the violet smell was perceptible in the urine, the turpentine was carried both into the stomach and lungs."

* However this may have been the intention of the author of the present essay, it certainly was not the case with some of the most strenuous opponents of cuticular absorption. EDITOR.

" A strong dog being fastened down upon a table, an incision, as in the former experiment, was made just above the superior extremity of the sternum, and at this place a ligature was passed around the trachea, which completely prevented the animal from respiring through his mouth. A small aperture was then made below the ligature into the wind-pipe, to which was adapted one end of a long tube, while the other end communicated with the mouth of a bottle containing spirits of turpentine. In this situation the dog continued to inhale by his lungs emanations of turpentine for two hours. The operation was then discontinued, the tube was removed together with the ligature about the trachea, and the incisions which had been made were closed: the mouth, fauces, and trachea of course resumed their former functions.

" The dog was now left from eleven o'clock, A. M. to half after six in the evening, when he was killed. The urine collected (a ligature having been previously passed around the penis for that purpose) gave a strong smell of violets. My obliging friend, Dr. Thomas Smith, and myself, made ourselves acquainted with this circumstance by repeated examinations." In addition to what is mentioned, it may be proper to state, that very particular means were used to prevent the animal from inhaling the turpentine by the mouth, both before and after the experiment. The turpentine was carried into the room in a bottle closely stopped; and, previous to the removal of the tube and ligature from the trachea, the dog was carried into another part of the house, where the smell of the turpentine was not perceivable, and in which place he continued until he was killed. This experiment, which was certainly conducted with all necessary fairness and precision, I must insist, very clearly proves absorption by the lungs, and that too under precautions which must have precluded " any portion of the

turpentine from having passed into the stomach entangled with the saliva." It in all probability has not been noticed, "in referring to the experiments detailed in the essays," or it would not have been asserted, "that the gentlemen cannot urge an objection against the one, which is not equally conclusive against the other, nor advance a fact which will not be as favourable for my opinion as for theirs."

Although I believe it will be allowed that the above experiment is very conclusive, at least with regard to the animal it was made on, yet the next one may be more satisfactory, on account of its having been performed on the human body. And as it can be repeated at any time without trouble, I hope every reader will do so for his own satisfaction.

On the 4th of April, 1809, having cleared my mouth and throat of saliva by spitting several times, I began to breathe the emanations of the turpentine out of a half gallon tincture bottle, during four deep inspirations, using the precaution not to swallow the saliva, after which I returned the stopper into the bottle, and again spit out all the saliva that had collected while inhaling the turpentine. After a minute or two had elapsed, I walked into the part of the room where the bottle was standing, and, on raising the glass stopper, I again inspired the turpentine an equal number of times, taking care to abstain from swallowing, and afterwards discharging the saliva by frequent spitting. This process I carried on for upwards of twenty minutes, alternately inhaling the emanations of the turpentine, and cleansing my mouth without having in any one instance swallowed my saliva. Before the time just mentioned had expired, my head began to ache, and I felt an unpleasant sensation in my breast. When three quarters of an hour were elapsed, the urine was inspected, and I think I was never sen-

sible of the violet smell in it to so great a degree. It was afterwards frequently examined; I think as often as five or six times, and on every trial the peculiar smell was more or less perceptible.

The reasoning made use of in the last number of the Museum, to explain why spirit of turpentine cannot be absorbed through the skin, though I perfectly concur with the writer that it cannot, I must be permitted to consider as unsatisfactory, since it would lead to a conclusion which is contrary to fact, viz. it would lead us also to deny its absorption from every part of the animal body. It has long been a very common, and, so far as I know, a very just saying, "it must be a poor rule which will not work both ways." If the supposed "numerous lymphatics which are spread over the surface of the body" are too fastidious to take up spirits of turpentine, it would be improbable they would be less high-minded in the stomach and lungs. Yet in those organs these dainty gentlemen do condescend to partake of "so acrid and stimulating," and consequently so unsavory a dish as spirits of turpentine. The lymphatic system, like other subordinate ones, has its own laws, and I trust these will not be found to contradict each other, or be essentially different in any of its parts. I do not believe that there are any absorbents which commence with open mouths from the surface of the cuticle. The cellular membrane is doubtless an extensive bed *for*, and may be it would be just to say *of* them; and they may originate from the cutis vera, agreeably to Mr. Cruikshank's opinion. They appear to be covered and protected from the noxious agency of all external causes, by a casement, or "a seven-fold shield," formed of an insensible, and I think, though it is offered as a mere matter of opinion, inorganic cuticle: of course it would be impossible for them to take up substances applied to the

sound surface, with which, from the interposition of the epidermis and rete mucosum, they could not come in contact. Situated as they are, they no doubt perform very important services in the animal economy, of which the occasional conveyance of fat from the cellular membrane into the general circulation, and of fluids from the cutis vera, which appears to be the organ of perspiration, perhaps constitute no inconsiderable part of their duty; and, when we advert to the admirable provision the Author of Nature has made for the protection of vessels so essential to the carrying on the important labours of the living machine, we must all feel sensible of an impulse to renew the truly contemplative and pious remark, "Fearfully, indeed, and wonderfully are we made, when such various channels of preservation from destruction so clearly point out the superlative wisdom of a Divine Architect."

With regard to mercurial ointment, the ingenious writer, in the last Museum, seems to have had experience of its absorption through the skin; which, if true, it must be confessed, would be unusual, and prove very readily the truth of the Galenic theory, which he advocates. This experience seems to be no less than "the evidence of his senses." Seeing, it has been said, is believing. I suspect there are not many writers to be found, who have deemed the impossibility of detecting mercury in the fluids of the body, as a conclusive argument against its absorption through the skin. This circumstance, as has justly been remarked, "can only be regarded as negative, not positive proof." "I must be allowed," he says, "to deny my faith to the extraordinary manner by which they suppose it to be introduced into the system, viz. by being carried in a state of volatilization into the lungs." And in the next passage but one, he says, "I certainly may be allowed to demand of them their proof of this assumed fact."

The following considerations, founded on experiments, will, I suspect, incline the most of readers not to conclude that the inhalation of mercurial vapours by the lungs is "either extraordinary, or an assumed fact." If mercurial frictions produce a salivation by absorption through the skin, then mercurial ointment, applied to it in such a way as to intercept all communication between it and the lungs by respiration, ought to excite a ptyalism just as soon as if no such precaution was used. The following experiments, however, made by Dr. Daingerfield, prove that this is not the case.

"To accomplish these important ends, I selected a substance, which, from the uniformity with which it has been supposed to produce its effects through the medium of absorption, promised results highly satisfactory. The substance alluded to is mercurial ointment. To ascertain whether it did or did not produce salivation, together with its other effects, in consequence of being taken into the general circulation, I applied large plasters of it to the calves of my legs; but, as I was apprized of the objection, that the mercury might be volatilized and taken into the lungs unless means were adopted to prevent it, and that, if applied to an abraded cuticle, the experiment would be inconclusive, care was taken to avoid both the one and the other of these sources of error, by applying them to a perfectly sound cuticle, and by covering them with thick bladders, rollers, and a pair of stockings, which were not removed until the end of the experiment. Thus circumstanced, I waited patiently the arrival of a speedy salivation; but, at the end of eight days, was completely disappointed, never having observed the slightest alteration in my general health, nor in my pulse, nor in the discharge of saliva. Not content, though, with this, and willing to believe that the result of my experiment had failed to correspond with my expectations, on-

in consequence of want of attention to a restricted diet, I accordingly resolved to live exclusively on vegetables, and to eat even of these with moderation. This determination was completely carried into execution, but with no better success; for, at the end of another week, the plasters had certainly produced no sensible effect. Unwilling, though, to abandon a doctrine so generally acquiesced in, and ardently desiring to know something conclusive on this subject, I reduced my system still farther, by the loss of fifteen or twenty ounces of blood, continued my vegetable diet, and applied, with the former precautions, two large mercurial plasters to *my fore arms*, where they were suffered to remain seven or eight days; and, at the end of which time, as they had produced no effect whatever, I put an end to my experiment, after its having lasted the greater part of three weeks." "From the foregoing facts," continues my indefatigable friend, "it appeared that there was no active power of cutaneous absorption; since, if there was, I ought to have been salivated, as having lived under every circumstance essential to that event. Afraid, though, to trust this conclusion to conjecture, when it could be made the subject of direct experiment, and apprehending that my experiments might possibly have failed from want of activity on the part of the absorbents to which the plasters were applied, or from some idiosyncrasy of constitution, or from some imperfection in my mode of living, or in the quality of the ointment used, I resolved to try the effect of frictions. Accordingly, on the same parts, with much less of the same ointment than had ever been applied at any one time in the form of plasters, I succeeded, in three nights, in gently affecting my mouth." The doctor goes on to observe, "from the foregoing experiments it would appear, that the absorbents stimulated by the friction used in the application of the ointment, were compelled to take it into the circulation. To determine, therefore,

how far this was really the case, it was necessary that the ointment should be applied to parts, which, at the same time that they possessed the advantages of friction, should leave nothing to apprehend from its being volatilized and taken into the lungs. Accordingly, I provided a strong pair of oiled-cloth socks, applied an ounce of unguentum hydrargyri fortius to the upper surface of each of my feet, put on the socks, drew a pair of stockings over them, and regularly walked a mile or two every day, that the friction of my boots against my feet might cause the mercury to be absorbed. At the end of ten days, I was greatly astonished to find that they had produced no effect. The socks were therefore taken off, and the same quantity of ointment again applied; after which they were renewed and permitted to remain sixteen or eighteen days longer; but, as in this time no alteration had taken place in the state of my salivary glands, they were again removed, and the experiment considered as concluded. It may not be improper to add, that my diet on this occasion was low, and strictly vegetable.

I trust it will be granted that mercurial frictions do produce a salivation in either the one or the other of the three ways by which their operation has been explained by different persons. 1. By absorption. 2. By sympathy. 3. By the inhalation of the mercurial vapours. Now, the establishment of any one of these contending theories must follow a refutation of the two others. I shall therefore proceed on the subject with this mode of reasoning. It will hardly be denied that the ingenious experiments of Dr. Daingerfield, which I have quoted at large, prove very clearly, that mercurial ointment is not absorbed through the skin; the question then recurs, how does the mercury pass into the system to occasion a salivation? I say pass into the system, because without its doing so, I be-

lieve a salivation cannot be produced. The doctrine of sympathy will not do, since the experiments related are equally obnoxious to it as to the hypothesis of cuticular absorption. The ointment was confined on the legs and arms the greater part of three weeks, without occasioning a salivation either by absorption or the sympathy of the nerves. I am well aware, in entertaining this opinion, I differ with Dr. Daingerfield, who has accounted for the operation of the mercurial ointment, by the doctrine of a sympathetic connection between the skin and the salivary glands: but the evidence of his own experiments related above, is so strong and so pointed in favour of what is termed "the volatile theory," that I am not prepared to agree either with the doctrine of absorption, or his of sympathy. The distance between the calves of the legs, or even that of the feet and ankles, and the throat, does not, according to my way of thinking, afford a reasonable explanation why the ointment did not salivate. Besides, "large mercurial plasters were also applied *to his fore arms*, where they were suffered to remain *seven or eight days*, and at the end of which time, as they produced *no effect*, he put an end to the experiment," &c. I do not hesitate to affirm, and I believe an impartial experiment would support me in the assertion, that if mercurial frictions on the feet and calves of the legs are persevered in a much less period of time than "ten days, or the greater part of three weeks," allowing a free access to the lungs, a salivation can be excited. In infancy, the policy or the government of the nervous system may not be sufficiently organized or accustomed to impressions, to establish at so early a period of animal life its different associated or "catenated motions," to use a Darwinian expression, and hence, perhaps, it may be correct to say, "children under a certain age cannot be salivated, because those two sets of vessels, the stomach and salivary glands, have not acted long enough together for their motions to become associ-

ated." But at the mature period of *adult life*, when it is presumable the various *customary* movements of the nervous system must have acquired a strong tendency to *habits*, it would not do to apply that doctine. Where there is no analogy, the analogical mode of reasoning will not answer, and it must be admitted, with regard to the associations of the nervous system, in " children under a certain age," and in adult life, the analogy is very slight.

It is thus I would oppose the doctrines of cuticular absorption and of sympathy, and deduce, as they are inadequate to explain the interesting fact in question, that the salivation must arise out of the evaporation of the mercury from the skin, and its consequent inhalation by the lungs. If a salivation cannot be produced by rubbing mercurial ointment on the skin, when the lungs are insulated, as Dr. Daingerfield's experiments prove, and as another experiment also does, which will be presently related, I ask, what is the conclusion that follows? Why, clearly, that the failure in producing that effect was owing to the mercurial vapours having been excluded from the lungs. But, in obeisance to the precept of the justly celebrated Bacon, " fiat experimentum," and, to use the language of my ingenious fellow-graduate, " afraid, though, to trust this conclusion to conjecture, when it could be made the subject of direct experiment," I shall proceed to detail what I humbly trust is a confirmation of it. The experiments were made about a twelve-month since. I shall extract them verbatim from an unpublished work on the subject of cuticular absorption. " In the first place, I ascertained that my mercurial ointment was of an active kind, and that in my constitution there existed neither temperament nor temporary irritation to oppose the production of a salivation. By one hour's rubbing of the ointment on the skin, twice a day, for three days together, a ptyalism

was produced. The present experiment was then commenced, by causing my boy, a young man, about fifteen years of age, to rub, briskly, the ung. hydr. fort. on the inside of my legs, an hour at a time, and twice a day. During each time the frictions were applied, every necessary precaution was used to exclude the mercurial vapours from the lungs, by breathing, in a long tube placed through the window, the pure air of out doors. As my system at this time was to all visible appearances in as susceptible a state to the operation of mercury as when a salivation was first excited, if the doctrine of cuticular absorption, or that of sympathy, be true, it would be reasonable to anticipate a salivation in three days from the beginning of the experiment. The frictions, however, were unremittingly continued to the sixth day; and, though no inconsiderable quantity of the strongest mercurial ointment was wasted at each time the frictions were used, yet nothing like a salivation was thereby produced. No tenderness or soreness of the mouth, no increased secretion of saliva, and no fœtor of the breath, ensued. At first, it was my intention to continue the experiment much longer; but the condition of my assistant on the sixth day, whose case I shall shortly describe, was such as to render a further continuation of the frictions improper. The more effectually to guard against the inhalation of the mercury by the lungs, whenever each hour of the frictions was completed, I constantly withdrew to another part of the house, and there had the ointment washed from the skin with soap and water; after which I again dressed myself, and usually renewed my attention to those professional duties which had a claim on me as a practitioner of medicine."

The next experiment is as follows. "I have mentioned that my assistant was so much indisposed on the sixth day of the last experiment, as to render a further continuation of it

improper. This case, which will constitute the present experiment, exhibited a set of symptoms very familiar to the physician. An evident tumefaction of the submaxillary glands, a soreness of the mouth, and a pretty copious excretion of saliva, made up the outlines of his indisposition. In short, he was completely salivated, though, from the very beginning of the frictions, precautions were used to prevent the mercurial ointment from coming in contact with his skin. He wore a very tight glove on the hand with which he applied the mercurial ointment, and the mouth of which was laced closely around the wrist. I acknowledge that the situation of my young man was much more favourable to the inhalation of the mercurial vapours than is usually the case with patients on whom the frictions are used; but the difference could not be so great as to admit of absorption by the lungs *only in his case*, and not in common ones. He usually sat down on the floor while he was rubbing the ointment on my skin, and the posture or inclination of his body was such, that, at every inspiration he made, the mercurial vapours were very plentifully applied to his lungs. On the sixth day of the experiment, as has been mentioned, he was salivated, notwithstanding the mercurial ointment did not come in contact with his skin."

These experiments, and those of Dr. Daingerfield, appear to establish, in a very satisfactory manner, the theory I have embraced, of the manner mercurial ointment rubbed on the skin produces a salivation. I think I am now sufficiently authorized to assert, that frictions of mercury do not occasion a salivation by absorption through the skin; because, when the ointment is applied separately to the external surface, though frictions of it be continued a period of time greatly exceeding that usually required to occasion a salivation, yet such an effect cannot be thereby produced. And I further conclude,

that the salivation is occasioned by the absorption of the mercury through the lungs; because, when the emanations of ung. hydr. fort. are copiously inhaled by them, a salivation ensues. Independent of the cogent reasons which have been deduced from the firm basis of experiments, in favour of the inhalation of mercury by the lungs, there is one more which at present occurs to me. It is a fact, that persons have been salivated merely by breathing for a few minutes the air of venereal wards in different hospitals which contained patients under the use of mercurial frictions. But if it were true that the heat of the body was insufficient to volatilize the mercury in the ointment, then such an occurrence could never have taken place. But it is said, " by exposure of ℥i of strong mercurial ointment, for two hours and a half, to a temperature of 120°, a degree of heat, *I* apprehend, seldom exceeded by the friction of the hand on the skin, I found it lost only fifteen grains, arising, *most probably*, from a loss of the sebaceous matter, and not of the metal." I must be permitted to assert, that, in my estimation, this experiment is entitled to no weight in deciding the question. Were the circumstances under which the ointment was exposed as favourable to its evaporation as frictions of it on the skin, the experiment might then be somewhat conclusive; but every one, at the first glance, must perceive that this could not have been the case. The particulars of the process are not mentioned; but, it is presumable, the ointment was exposed, in a cup, to 120° of heat, and the first effect which ensued must have been the liquifaction or melting of the lard. The divided or triturated mercury, on account of its gravity, would, in a short time, settle to the bottom of the vessel, and the supernatant lard, in the form of oil, would effectually prevent its evaporation into the surrounding air. Hence the small quantity of the ointment lost during the two hours and a half is not to be wondered at; and hence,

the supposition, that this loss consisted " of the sebaceous matter, and not of the metal," is not improbable. When mercurial ointment is briskly rubbed over the skin, its evaporation is greatly promoted by the extent and the thinness of its successive layers, which are equally acted on by the animal heat and by the free access of atmospheric air. But these favourable agents to evaporation, which always operate in the use of mercurial frictions, are prevented from exerting their usual influence in all such experiments as the above.

The matter of many contagious diseases, contracted in the casual way, seems to be principally conveyed into the system through the lungs. Hence, perhaps, the very considerable affection of the lungs in measles, and hence perhaps the reason the matter of small-pox will neither infect the system when applied either to the sound skin, or when taken into the stomach. I do not assert these facts from my own experience, though I do not hesitate to credit them. That of the non-absorption of the variolous infection by the skin, I believe, was first ascertained by Dr. Benjamin Rush, and the other fact originated with a physician in the state of Jersey, whose name is not at this time recollected.

The characteristic properties of the infection, it is presumable, are destroyed by the digestive powers of the stomach. The same vital or assimilating power is exerted, and the same change takes place, when the poison of the viper, and the matter of syphilis, are taken into the stomach. As is the case with the contagion of small-pox, those violent causes of disease and death are rendered harmless when taken into the stomach.

The late Dr. Currie was an advocate for the conveyance of contagion into the system through the lungs. This highly respectable author says, " if the non-absorption by the surface of the body be established, it will ascertain that, in the ordinary course of things, contagion is received into the system by the lungs only, and will justify a practice which is common among our more experienced seamen on the coast of Guinea, and other warm climates, who, when exposed during the night to a breeze from the marshes, wrap their heads in a sea-cloak, or other covering, and sleep fearless on the deck, with the rest of their bodies nearly exposed." The primary operation which the lungs and trachea sustain from the contagion of casual small-pox may be a principal cause of that noted liability of those parts to become violently and often fatally affected with this disease, at that stage when, in popular language, the pock begin to turn.

This affection of the lungs seems to partake greatly of the familiar character of the common pneumonic affections. In some cases, I think I have perceived pneumonia vera, and sometimes pneumonia notha, to succeed about the turn of the pock. If I am right in the character of this dangerous succeeding affection of casual small-pox, the propriety of using the lancet, a salivation, and other evacuating remedies for its removal will perhaps be made as reasonable *in theory* as they have heretofore been found successful *in practice*.

It may be inferred that the almost continued irritation in the lungs, so characteristic of measles, is owing to the primary operation of the infection on them, from the fact that this complaint, when produced by inoculation, is nearly or quite unattended with that mark of disease.

Dr. Wilson observes, "the chief difference between the casual and inoculated measles seemed to be the absence of any pulmonic affection at all periods of the latter."

With regard to Mr. Home's experiments, I neither am inclined nor do I conceive it necessary to say much. Not inclined, as his theory does not affect pulmonary absorption; nor necessary to say much, as this is not the place to discuss his hypothesis. His object, if I comprehend him rightly, is to establish the existence of a vascular connection between the stomach and bladder, through the spleen, unconnected with the common rout or the general circulation. It may possibly be, that a secret channel of this kind exists; but I must confess the theory seems to labour under many difficulties.

A few years since, an experiment was made in this city by Dr. Rogers, then of Lexington, and myself, to ascertain how far Darwin's theory of an occasional conveyance of fluids from the stomach into the bladder, by his supposed inverted motion of the urinary lymphatics, was well founded, which, though at that time we could not have Mr. Home's idea at all in view, as he had as yet perhaps not conceived it himself, I think will not only reflect great doubt on Darwin's doctrine, but also to an equal degree on Home's. I shall briefly relate it. An incision was made into the lower part of the abdomen of a dog, when the ureters were found, and a ligature passed around each of them within two or three inches of their entrance into the bladder. The urine contained by the bladder was then pressed out with the fingers, and, in order to retain in the bladder whatever might afterwards be deposited in it, a ligature was passed round the penis. The wound occasioned by the incision was closely stitched up, when we commenced giving every now and then a dose of gin, to produce that inebriation, which Dr. Darwin con-

ceived very conducive to invert the motion of the urinary lymphatics. After a while we gave the animal, in the gin, madder and nitre, two articles well known for producing a considerable effect on the urinary system. We dosed the devoted animal in this way for an hour or two, until we were of the opinion a sufficiency had been exhibited, when he was left about eleven hours in a room. I pithed him with a scalpel, and on opening the wound we found the ureters above the ligatures much distended with urine up to the kidneys, but not a drop of this fluid existed below, either in the ureters or bladder. This experiment induced Dr. Rogers and myself to conclude, that the ingenious author of the celebrated Zoonomia was mistaken; and as it has never been published, I now offer it to the public, not only for the single purpose it was first designed, but as an experiment showing that a direct channel through the spleen from the stomach to the bladder in all probability does not exist. Did it exist, the rubia tinctorum impregnated with nitre would have certainly made its way into the bladder in spite of the ligatures about the ureters.

I communicate the experiment and use the name of my friend without consulting him; but, from my knowledge of his love of investigation, I have no doubt of its meeting with his approbation.

Tying up the ureters previous to the introduction of coloured substances into the stomach, must appear like a much more fair as well as conclusive method of experimenting to ascertain this supposed channel, than the one used by Mr. Home. The only experiment in his communication which in the least wears the appearance of being unequivocal, is the one in which the pylorus was taken up. But, if even this be closely examined, it cannot justly retain such a conclusive character in the mind of

the scrutinizing and candid physiologist. If those direct vessels do exist, they doubtless are susceptible of having either false offices imposed on them, or their natural ones suppressed, by severe injuries; and which serious injury every one must perceive in tying up with a ligature so sensible an appendage to the stomach as the pylorus. Again, Mr. Home's experiments, granting they prove the existence of the secret passage in question, do not nor could not acquaint either himself or us with the part of the urinary system to which substances are carried. They might be discharged either in the bladder or ureters, or exclusively in the kidneys. But my experiment proves, and I must be entitled to draw a conclusion as I believe it will always succeed, that fluids are not carried from the stomach into either the lower part of the ureters, or into the bladder. The fact of substances showing themselves in the urine in the course of a short time after their introduction into the stomach, their disappearance for a while and again returning, I am ready to admit, stamps on Mr. Home's hypothesis an air of plausibility. And when I admit this, I must be permitted to caution against mistaking mere plausibility for *positive proof*. Though rather improbable, yet I believe it will be admitted that such occurrences may successively ensue through the well known and long since established channel of the general circulation; at any rate the fact stated cannot prove to conviction that a more direct channel does exist, such as Mr. Home has imagined. My promise to say not much in this place may attract observation after what I have said, but, compared with the importance of the inquiry, I in reality have said but *little*. Mr. Home has opened a new field of researches, and I believe *much* remains yet to be done. The science of medicine is yet rich with extensive fields of terra incognita, and the mere apprisal of the quarters in which they lie can only form the beginning of a series of important discoveries.

A Pathological Fact, in proof that there exists a more direct Channel of communication between the Alimentary Canal and the Bladder, than that through the medium of the Sanguiferous System. In a Letter to the EDITOR *from* MAXWELL M'DOWELL, *M. D. of Baltimore.*

Baltimore, February 14*th*, 1809.

SIR,

IF the following singular pathological fact should appear to you worthy a place in your Museum, it is at your service. I am, sir,

Your humble servant,

MAXWELL M'DOWELL.

Dr. J. R. Coxe.

Medical philosophers have, for a long time, suspected the existence of a much nearer *channel* of communication between the alimentary canal and bladder, than the circuitous route of the blood-vessels. Numerous physiological and pathological facts have given rise to this suspicion. Ingenious experimentalists have endeavoured to make suspicion yield its seat, in the human mind, to the clearer evidence of *established* fact. The knife of the anatomist has also been employed in the important investigation; but it has not thrown any other light upon the subject, than what is reflected from a discovery in *comparative* anatomy. The ingenious and ever to be lamented Mr. Hewson discovered in a turtle, "that the lacteals near the root of the mesentery anastomose, so as to form a net-work, from which several large branches go into some considerable lymphatics lying near the spine, and which can be traced almost to

the anus, and particularly to the kidneys." The late Dr. Darwin, supposing that the lacteals in the human system were connected with the urinary branch of lymphatics by anastomosing branches, undertook to account for the communication in question, by his doctrine of the " retrograde action of the urinary branch of the lymphatic system."

The subjoined fact, which took place in my own system, satisfies my mind beyond the *shadow* of a doubt, that the suspected communication *actually* exists.

Early in the autumn of 1798, being in a small village in the state of Kentucky, I felt slightly indisposed, and considered it necessary to take a cathartic. I swallowed *half a drachm* of pulv. rhæi. When I felt the action of the medicine in my bowels, I retired to the forest, for the village did not contain *one* temple dedicated to the goddess Cloacina; and to that circumstance alone I am indebted for the pathological discovery. The first evacuation was *per urethram*, and the extraordinary colour and consistence of the urine produced a momentary alarm, which yielded to a regret that I was not furnished with a vessel to secure the urine for examination. The urine, in passing over the leaves, left an *earthy* matter attached to them. The next time that the medicine required me to retire, I put a four ounce vial in my pocket. I filled the vial with the first urine that appeared, which retained a slighter shade of the same colour of that which preceded it, and was also less turbid. After letting the vial stand a sufficient time, in a secure place, I poured the liquid off the deposited substance, suffered it to dry, and then ascertained, to my entire satisfaction, that it was a portion of the pulv. rhæi, which I had swallowed a few hours before this turbid urine passed off. It may not be amiss to mention, that, though I had two copious *alvine* eva-

cuations, yet their consistence, together with their manner of expulsion, seemed to show that the rhubarb had no other effect upon the bowels, than *moderately* to increase their natural peristaltic motion.

I have mentioned the foregoing fact to several of my medical brethren. Some of them suggested that the deposited substance might have been a *lateritious* sediment. To such I beg leave to propose the following questions: Would urine, in passing over leaves, leave a *lateritious* sediment behind? Does the *passing* current of urine, containing *lateritious* sediment, appear *turbid?* Is it not remarkable that my urine should not have deposited a *lateritious* sediment at any former period of my life? Is it not equally strange that my urine has never been *turbid* since the autumn of 1798? In fine, is it not unaccountable that the only period of my life, in which my urine deposited a *lateritious* sediment, should be *four* or *five* hours after I had swallowed *half a drachm* of rhubarb? About three years ago I took a *scruple* of rhubarb, in order to see if it would pursue the same course; but it passed through my bowels by purging me briskly. The state of my system, however, when I took the scruple, was very different from what it was in the autumn of 1798. I am entirely at a loss to know how the rhubarb found its way into my bladder. Perhaps the communication between the alimentary canal and bladder, in my system, is a "lusus naturæ." Of one thing I am *certain:* the rhubarb did pass off with my urine, for, before I can disbelieve the fact, I must be taught to discredit the testimony of my senses of SIGHT and TASTE. Surely no man, in the dispassionate exercise of his intellectual powers, could for a moment suppose that the rhubarb entered my left subclavian vein, passed on through the right auricle and ventricle of the heart, performed the round of the pulmonic circulation, entered the

left auricle and ventricle of the heart, and continued its course along the descending aorta, to be conveyed out of the system through the emulgent arteries and urinary organs. Such a supposition would, in my opinion, be an outrage against common sense. Till the knife of some fortunate anatomist dispels the cloud which, at present, envelopes the subject, the only resting place I can find, for my mind, is that which Dr. Darwin has suggested. I must therefore think that a *diseased* action of my lacteals took the rhubarb out of the alimentary canal, and that anastomosing branches conveyed it to the urinary lymphatics, and that it entered my bladder by means of the "retrograde action of the urinary branch of the lymphatic system." Of the truth or error of this hypothesis "*Dies doceat.*"

An Account of a Case of Diseased Uterus and Ovaria. Communicated to the EDITOR, *by* WILLIAM BALDWIN, *M. D. of Wilmington, in the state of Delaware.*

October 28th, 1808.

TO endeavour to remove the diseases, and to alleviate the sufferings, of our afflicted fellow-beings, are duties of the physician. To fulfil with effect these duties, we should carefully record all extraordinary cases, by which means every possible advantage may be derived from them, in directing others, where we have failed, to more successful modes of practice, and, where we have succeeded, to follow our example: and thus it is that the *healing art* must continue to be extended and improved. The imperfections of *medical science* remain to be deplored. *The hydra, Disease, in some of its various forms, still baffles all our efforts, and triumphantly bears off its trem-*

bling victims prematurely to the grave! But we ought never to despair. We are equally unacquainted with the extent to which our faculties may be improved in our assiduous investigations, as with the abundant means that yet remain concealed from our view, in the wide field of *medical science*. Let the brilliant successes of a *Darwin*, a *Jenner*, a *Rush*, a *Barton*, and many others, stimulate us to new and more vigorous exertions in the cause of suffering humanity. Tetanus, hydrocephalus internus, and phthisis pulmonalis, have all of late yielded to the power of medicine; and when we reflect that it is not very long since an *intermittent* was considered as an *opprobrium* in medicine, we may very reasonably hope that some or all of those diseases, which still continue to mock our efforts, may, at some future period, yield as readily to the power of medicines yet unknown, as an intermittent now yields to the salutary powers of the officinal cinchona.

Judy Delany, a negro woman, married, and aged about forty years, was born a slave at Newport, in this state, but was set free at the death of her master, some years after. About ten years ago she became pregnant, as she supposed, with her first child; but through the whole course of her supposed pregnancy she continued to menstruate regularly. At the end of nine months she experienced for a few days considerable pain, always accompanied with a *bearing down*, which would recur, as near as she could recollect, every half hour. Her breasts filled with milk, which afterwards gradually disappeared. It was the general opinion, both of physicians and others, at this time, that she was pregnant. Her abdomen continued to be distended, but it remained nearly stationary for several years, and she enjoyed such a share of health as enabled her to labour for subsistence. Five years ago an examination was made by a respectable physician, who could easily introduce his finger into

the *os tincæ*, and substances like *hydatides* presented to the touch. She continued in this situation, her abdomen having gradually increased in size, and she had applied in vain to a number of physicians, till last summer, when she was admitted into the Newcastle county infirmary, near this borough. On being examined by several physicians, the *os tincæ* was discovered to be completely closed, and a hard round substance appearing to the touch like the head of a child was detected in its place. The abdomen was greatly distended, and had of late encreased in size more rapidly. The menses still continued to flow at regular periods, but sparingly; and she informed us that they had never ceased to flow. There was nothing remarkable in the state of her pulse, her appetite was generally pretty good, and her bowels mostly regular.

Mercury was made use of till the mouth was considerably affected, afterwards the external application of tobacco to the abdomen, with a variety of other remedies, but all to no purpose.

The bodily pain which she endured was inconsiderable, but she was under the most distressing apprehensions, from an idea that her womb was filled with living *monsters* of hideous and terrific forms! She was further confirmed in this superstitious opinion from a tremulous spasmodic motion that frequently took place in her abdomen, and which indeed bore some resemblance to the motion of the *fœtus in utero*. She had been under this alarming impression since the termination of the first nine months, at which time she fancied herself so unfortunate as to come under the *dreadful influence of witchcraft!* To shake her faith in this belief, no effort of reason could avail, and she enjoyed a temporary respite from the most

agonizing feelings, under the management of a black man, who, she informed us, had so far stilled the *demons* that haunted her, as that she could sleep much better at night!

The disease continued to progress: her thighs and legs became swelled and *œdematous*, particularly the right; and from the fluctuation that was perceivable, there was now reason to apprehend a collection of water in the abdomen, whatever might be the state of the uterus. But she was still able to walk about; was not emaciated, and her countenance had not that appearance which usually characterizes dropsical patients. Her appetite continued to be pretty good, but she could take but little food at a time, owing to the pain which it produced. A table spoonful of pepper, she informed us, often relieved the pain she felt after eating, and kept the *monsters* down! after which she inclined to be sleepy. The right side of the abdomen was rather the most distended and hard, particularly about the region of the liver, where she complained of some pain.

She continued to walk about till within a few days of her death; and could lie without difficulty in a horizontal position to the last.

On the 20th instant, she was seized with faintiness and vomiting. Her pulse was low and feeble. Some volatile alkali and laudanum were administered, which stopped the vomiting; after which she lay easy and sensible of her situation till midnight, when she quietly expired.

The body was carefully examined on the following morning, in the presence of *Dr. Monro*, and several other respectable physicians.

The extent of the tumour, from the *anterior superior spinous process* of one *os ilium* to the other, over the most prominent part of the abdomen, measured twenty-five inches; and from the *symphysis* of the *os pubis* to the *scrobiculus cordis*, thirteen inches, as she lay extended on her back.

From an idea that the abdomen contained a quantity of water, a puncture was first made in the right side with a scalpel, which penetrated the *uterus*, from whence was discharged the enormous quantity of near six gallons of a fluid resembling pus! It was of a pale dirty colour, slightly tinged with red, and emitted a disagreeable fœtor.

On laying open the abdomen, the *uterus* and its appendages were the first objects that solicited our attention. The *uterus* appeared to have occupied the whole cavity of the abdomen and even a portion of the thorax, as the diaphragm was totally obliterated, and the liver chiefly confined in the last mentioned cavity. It adhered slightly to some of the small intestines, and was in many places *scirrhous*, particularly for several inches round the *os tincæ*, which was as completely closed as though no orifice had ever existed. On cutting through into the cavity of this part, the thickness was found to be two inches. At the place of separation from the *ovaria* we cut through an extent of *scirrhus* not less than three and a half inches in length and two in breadth. We found a number of fleshy substances floating in the cavity of the *uterus*, completely detached, of a soft spongy texture, the largest of which would probably have weighed at least half a pound. The internal surface had a rough jagged appearance; the external was smooth. It weighed, when completely evacuated, and separated from the *ovaria*, six pounds.

The *ovaria* were greatly enlarged. One of them was soft, and upon being laid open was found to contain a white fluid substance resembling curdled milk. It was perfectly free from smell, but imparted a slight sense of pungency to the taste. The other was very hard and white. The *ova*, to the number of fifteen, were more or less enlarged, and there were several others of inferior size. The *fallopian tubes*, and the ligaments were all lost in this deformed mass, which weighed five pounds and a quarter! The spleen, liver, lungs, heart, and indeed the whole of the contents of the abdomen and thorax, had a natural healthy appearance. The stomach was remarkably contracted, but exhibited no mark of disease.

This account shall be closed with a few observations and reflections.

The *uterus* and *ovaria* are liable to be affected with scirrhus, cancer, dropsy, polypi, tubercles, &c. but such a combination of these morbid affections, prevailing to such an extent, and for so long a time, has perhaps seldom been met with. The opinion of pregnancy that was first entertained by the patient and others, was, in all probability, fallacious; and the symptoms that favoured such an opinion were the effect of disease, influenced, perhaps, in some degree, by the state of the patient's mind. Those spongy substances found in the cavity of the *uterus* were probably *polypi* or *tubercles;* and the jagged appearance of the internal surface of that organ, with the state of the fluid therein contained, pointed out the existence of *cancer*. It is a remarkable fact, that the discharge of the *catamenia* continued throughout the whole course of the complaint, notwithstanding the mouth of the *uterus* was so effectually closed. Does not this circumstance prove that the discharge is from the *vagina* instead of the *uterus?* It is perhaps impossible to

account for the destruction of the *diaphragm*, and to satisfy ourselves of the reason why a muscle considered of such importance in respiration could be spared without any inconvenience, as the want of it was not perceived; no difficulty of breathing having occurred, either in an erect or horizontal position! Might it not have been absorbed from the pressure of the *uterus?* The motion which our patient complained of, and which might readily be felt by the hand, proceeded, no doubt, from contractions of the distended *uterus*, and might be in some degree influenced from the superstitious idea she entertained of being full of living animals, as she was relieved by remedies which acted through the medium of the mind.

This case affords us a striking example of the extent to which *disorders*, or local affections, may proceed, without inducing *disease*, or general morbid action. A scratch with a bone on the finger, and the puncture of a nail in the foot, have in a few days terminated in death, from general morbid action being induced; while we find that *cancer* and *dropsy of the uterus*, with *scirrhous ovaria*, may exist for many years, without producing *disease*, or injuring materially any of the animal or vital functions.

Observations on Cold, by Dr. GEORGE WILLIAMSON. *In a Letter to the* EDITOR.

Baltimore, December 1st, 1808.

DEAR SIR,

IN the several last numbers of the Museum, we were favoured with some ingenious speculations and facts on cold. Both of these writers, no doubt, think themselves correct; and I will not adopt the language of the poet, and say, they are "each claiming truth, and truth disclaiming both;" nor yet, that my sentiments are more correct than either of theirs. It may be thought officious in me to address the public on an important subject, which is under discussion by those of superior information. But my wish is not to intermeddle with these; nor do I purpose to enter into an examination of their sentiments, further than as generally received by those who espouse either of their opinions. I am not fond of controversy; too many, who are, have access to irritating and abusive language. To be both disbelieved and abused is more than I am willing to be subjected to: nor am I vain enough to suppose that my information on the subject in question is superior to that of my fellow professors; nor that I shall be able to make use of more powerful arguments than those who have already written on it. But I wish, in as conspicuous a manner as in my power, to present the reader my sentiments; and I intreat him to examine them maturely. If this is done, and done with an unprejudiced mind, he will, if correct, adopt, but, if erroneous, reject them. To me, it would be a source of great pleasure to be irradiated with the superior information of other gentlemen on this or any other subject; and if I possess any information on this subject which my fellow practitioners do not, I wish to impart it to them, because I think it i cur profession

of much importance; and it is very desirable that it should be correctly understood, and not that there should be such a diversity of sentiment. This diversity of sentiment deters many from having access to one of the most valuable remedies we are in the possession of; and should they have access to it, unless they know upon what principle it acts, they may be right, but it is quite as possible for them to be wrong; if they should err, their error is frequently of an alarming if not a fatal nature, as will be proved by and by. Although I am anxious to become completely acquainted with every thing relative to our profession; and not only myself, but that all should who undertake the practice of medicine, and have the lives of their fellow beings entrusted to their care; yet I have so little solicitude for the adoption of my opinion, that, should any person think proper to reply to this essay, I shall be silent, unless I think I can adduce something new; and even should I suppose I can further elucidate the subject, shall not notice an anonymous writer. An anonymous controvertist I consider as an enemy in ambush: if he gains a victory he soon exhibits himself, at least to his friends; but, if unsuccessful, skulks off under the veil of obscurity, and when out of all danger is very apt to join with those who censure his production.

I shall first make a definition which I wish to be understood as my opinion, that I may be comprehended, and that there may be no useless display of words about an undefined proposition.

Every thing that directly reduces the excitement is a sedative.

I shall next lay down two propositions, which I consider incontrovertible, and which are, I believe, generally received.

1st. Caloric, or matter of heat, has always a tendency to support an equilibrium*.

2d. There is in the cellular membrane and muscles an inherent power, which produces contraction when the excitement that expands them is removed.

First. Every thing which directly reduces the excitement is a sedative.

I shall now endeavour to prove that cold directly reduces the excitement. In prosecuting this part of my subject, I shall, in compliance with usage, adopt the term cold in its general acceptation. I am, however, aware, that in speaking of any thing, to say that it has such and such effect is granting it to be an active agent; and that every active agent is a stimulus. It is therefore to be regretted that a term which is so highly improper, as tending to deceive, and distract the judgment, consequently occasion irreparable injuries, should not, at least, be expunged from medical language.

I do not deem it necessary to enter into a detail of facts which are obvious to the most superficial observer, and which are acceded to by those who most strenuously advocate the doctrine of cold being a stimulus: but shall proceed to notice some which, to me, appear more conducive to the support of such an opinion, than any others with which I am acquainted, or that have been advanced by its advocates.

1st. After coming out of the cold bath, there is a glow on the skin, with increased heat, and the pulse is accelerated: but this does not prove cold a stimulus, for cold does not pro-

* Unconfined caloric.

duce this effect. If cold does not produce this effect, the advocates of it being a stimulant will ask, what does? For their satisfaction, I will endeavour to prove it owing to heat. We will suppose the temperature of the atmosphere which surrounds us to be 75° of Farenheit, the bath 60°, and that of our system 96°. When we go into the bath, a diminution of excitement takes place; consequently an accumulation of excitability. While in the bath, if composed, the pulse is less frequent, &c. but on coming out of it into an atmosphere from ten to fifteen degrees warmer than the water we have just left, and that increased temperature operating on an excitability several degrees accumulated, it must necessarily produce an augmented excitement; for heat is a stimulus, and every increased degree of heat increases the excitement, if the attendant circumstances are such as they were previous to this increase. It may now be said, if I do not suppose cold to act directly as a stimulus, that I do indirectly. Objections of this nature would divert me from my proposed plan; therefore, in reply to it, I must first say, that on the present occasion I wish to wave all indirect causes and effects. Second, to say, because heat operates more powerfully on a system whose excitability has been accumulated by cold, that this is a stimulus, would be no more consistent, than to say, ether, spirits, &c. are sedatives, and therefore should be used as such in cases of increased excitement.

2d. Cold is a sudorific; but this does not prove cold a stimulus. When a person labours under a great increase of excitement, if there is an exhalation from the cutaneous capillaries, the evaporation is so rapid as to prevent the aqueous particles from concentrating and remaining long enough on the skin to be discovered. But when a diminution of the excitement takes place, the evaporation is less rapid, and a deposi-

tion on the skin is the consequence. This diminution may be easily accomplished by cold: hence cold is a sedative. In some diseases, it may be said, the skin is preternaturally hot, and there are no obvious symptoms of a diaphoresis; but so soon as one breaks out, the patient is much relieved; and this is accomplished by cold. Such may be the fact, but it does not prove cold a stimulus. If cold is applied externally, there ensues an expenditure of heat, the skin becomes cool, the pulse less frequent, the system is reduced to the sweating point, and it frequently succeeds. When used internally, although there may be no very evident symptoms of increased excitement, except that of the skin, yet it frequently excites perspiration; not from any stimulating property, but perhaps on the principle of sympathy. The most of those preternatural affections of the epidermis are owing to some disease of the primæ viæ. The skin has been compared to an inverted sac, not only extending over the exterior of the system, but also through the interior, and is everywhere furnished with nerves, the vehicles of all impression. We may, no doubt, to these circumstances attribute, in a great measure, that close connection and mutual influence which is well known to exist between the skin and stomach, &c. and it is also, I suspect, owing to this that cold water, when taken internally, so frequently acts as a sudorific. However, if the doctrine of association should be rejected, that of our general principle is sufficient; and the simple fact which takes place in health, on taking a glass of cold water when the system is much heated from exercise or any other means, establishes it beyond a doubt.

3d. Cold sometimes accelerates the pulse; so does bleeding. This is not a stimulus, neither is that. In this respect, I think the use of the lancet and that of cold very analagous. It is in cases of oppressed pulse that the lancet accelerates it. If

cold ever directly has this effect, it is under similar circumstances. They both lessen the excitement, and are both sedatives.

4th. Cold excites absorption. Perhaps it would be more proper to say, cold prevents suppuration. Heat is a stimulus, and increases the vascular action; and an increased action is necessary to suppuration. Any thing that diminishes heat diminishes the action; consequently retards or prevents suppuration. Blood-letting reduces the vascular action; so does cold. That is not a stimulus, neither is this. Cold is also very friendly to transpiration, especially where there is preternatural heat, and has, by this means, a very powerful effect in preventing suppuration. Independantly of its equalizing the temperature of the general system, much of that heat of the phlegmon, &c. is expended by this process. I think it very questionable whether cold promotes absorption further than by preventing suppuration. When in a healthy state, the absorbents are engaged in performing their proper function, which is to absorb fluids from the different parts of the system, some of which are necessary to health, and others offensive; and, were they not removed, would excite morbid action. In cold indolent tumours, where electricity and other stimulating applications are highly useful, does any good accrue from the use of cold? If so, is it not by accumulating the excitability, and enabling subsequent stimulants to act more powerfully? are not the absorbents susceptible of increased action, and may not those fluids which are thus deposited, and which they remove, be capable of exciting this increased action, especially after an accumulation of excitability?

PROPOSITION II.

"Caloric always endeavours to obtain and support an equilibrium; not that it is equally distributed among all bodies, but is dispersed among them according to their degree of mutual affinity." It is supposed by many philosophers that caloric and animal heat are similar. Oxygen supports both, and neither can exist without it*: they are both very diffusive, and always tend to an equilibrium. Doctors Beddoes, Thornton, and others have written ingeniously and lucidly on this subject, and their conclusion is, that "animal heat proceeds from the chemical union of certain parts of our food and oxygen, modified and combined by the proper exercise of the natural animal functions disengaging caloric." That animal heat always tends to an equilibrium may, perhaps, be doubted. 1st, It may be said, that in a state of health, some parts of the system will be cold, while others are warm. This is not owing to any disposition in the animal heat to pervade some parts of the system and not others, but to collateral circumstances. In cold weather, if one part of the system is more exposed than another, the part so exposed parts with some of its heat to the atmosphere. If a part thus exposed perspires more than another, part of its heat is expended by exhalation; and, if any collateral circumstance favours this process, the expenditure is increased.

* Dr. Beddoes, in a letter to Dr. Darwin, in speaking of the effect superoxygenated air had on himself, says, "In no long time I observed in myself a remarkable power of sustaining cold. Except one or two evenings I never experienced the sensation of chilliness, though cold easterly winds prevailed during great part of the time I inspired the superoxygenated air." He adds, "I was not only able to reduce my bed-clothes to a single blanket and coverlid, but slept without inconvenience in a large bed-chamber, looking to the north-east, with the window open all night, and with the door and window of an adjacent sitting-room also open."

If on a cold day we wet our hands, or if in travelling in a cold rain, with gloves on, that receive and retain water, our hands will be much colder than if they were kept dry, or even than if the gloves were taken off. The Arabians, when conducting caravans over the deserts, have the following remarkable method of cooling their wines. They dig a hole, and, having filled it with straw or clay, they place the bottle of wine they mean to cool into the midst of it, having previously surrounded it with wet straw or clay. They then set fire to the straw, and the bottle of wine is brought out (from the evaporation of the wet clay or straw surrounding it) quite cool. Vide Philosophical Transactions, Vol. LXV, p. 252. Other nations cool it by hanging up their bottles in wet cloths to the sun, to expedite the evaporation; the cooling of the wine goes on in proportion to the quickness with which the heat is abstracted by evaporation. It is on this same principle that cold applications are so very salutary in local inflammation, and in some inflammatory diseases of the encephalon.

Our illustrious countryman, Franklin, proved that perspiration always produces a certain degree of coldness. See his letter to Dr. Lind. "We find that almost all fevers end in perspiration; which, besides the advantage of expelling the morbific matter, possesses likewise, that of carrying off the matter of heat, and restoring the body to its common temperature." *Chaptal.*

That almost all fevers end in perspiration is too general an assertion; but that the most of those do, which do not terminate in death, or in which the excitement has not been reduced by some depleting remedy, is an unquestionable truth.

"If heat be applied to water, these two fluids will unite, and the mixture will be dissipated in the atmosphere; but it would be an abuse of words to call so weak a union by the name of combination; for as soon as heat becomes in the situation to combine with other bodies, it abandons the water, which returns to a liquid state. This body, during evaporation, continually carries with it a portion of heat; and hence, perhaps, result the advantages of transpiration, perspiration, &c." *Chaptal.*

According to my position it may be said, that the animal heat must be different in different climates. That the climate has a material effect on the animal system is a truth well known. Dr. Robertson, in his History of America, says: "In every part of the earth where man exists, the power of climate operates with decisive influence upon his condition and character. In those countries which approach near to the extremes of heat and cold, this influence is so conspicuous as to strike every eye. Whether we consider man merely as an animal, or a being endowed with rational powers which fit him for activity and speculation, we shall find that he has uniformly attained the greatest perfection of which his nature is capable, in the temperate regions of the globe. There his constitution is most vigorous, his organs most acute, and his form most beautiful; there too he possesses a superior extent of capacity, greater fertility of imagination, more enterprizing courage, and a sensibility of heart which gives birth to passions not only ardent but persevering. In this favourite situation he has displayed the utmost efforts of his genius in literature, in policy, in commerce, war, and all the arts which improve and embellish life." These inferences were drawn from actual observation, and are consistent with those of other historical travellers.

That animal heat is influenced by climate can hardly be doubted. Climate has considerable influence on the pulse, and a mutual influence between animal heat and the circulation is acknowledged. Although there are these connections, and although the climate has considerable influence on the pulse, yet it is not in arithmetical progression; nor is there that difference of animal heat in different climates that we might at first suspect, owing, I imagine, more to artificial than any natural cause.

This opinion is rendered at least plausible, from abundant experience, not only in man, but also in inferior animals. In climates colder than the temperature of the system, men have access to clothing, &c. to prevent such an escape of heat from the body as would be unpleasant or injurious; and He who regards the least of his work, has furnished inferior animals with a warmer covering in those situations than in warmer climates. Their covering is not only warmer, but is also a bad conductor of heat. Hunter endeavoured to freeze a dormouse; but at first found he could only freeze its feet. Supposing the hair to be a bad conductor of heat, he, in his second trial, took care to wet it all over: after this he succeeded.—*Hunter, on the Animal Economy.*

Feathers, when dry, are also bad conductors of heat. Birds will fly through very cold regions, when dry, more so, perhaps, than when wet. And yet Providence has not been unmindful of aquatic fowls. The effect cold rain has on the common dung-hill fowl is strikingly obvious.

There is also a peculiar economy observed by man and inferior animals to prevent, in cold weather, too great an expenditure of heat: observe, for instance, the dog. If the weather

is very hot, he extends himself in the shade, his mouth wide open, and respiration hurried. If the weather is very cold, he endeavours to get as near as possible to a fire. If this cannot be accomplished, he gets into the sun, or the warmest place he can, and contracts himself into the smallest possible dimension, knowing, from painful experience, that the more he is extended in cold weather, the colder he will be. These observations are also applicable to the cat, &c. The hog, in hot weather, hunts for a mud-hole or water. When they are wet, the expenditure of heat is much increased; in cold weather they huddle together in heaps, having experienced that there is more heat in several than in one. In fact, there is no part nor inhabitant of the terraqueous world, which comes under the operation of severe cold, that does not show, conspicuously, the print of her congealing hand.

Some substances conduct heat less rapidly than others; so do some animals: those that do not perspire impart with it less than those that do; and those that do not impart much heat this way, impart more by respiration than those that do; and "we know that the heat contained in one breath of air, will, if properly managed, raise Farenheit's thermometer ten degrees*."

Animals retain the heat while in a contracted state, and expend it while extended with the mouth open. Horses, dogs, &c. in cold weather, have access to muscular motion: such as running, leaping, playing, &c. which increases respiration and heat. Muscular motion has the same effect with man.

* Vide Critical Review, for January, 1782, page 6.

The weather has also great influence on the retention and expenditure of animal heat. If the weather is dry and calm, the expenditure is much less than in windy wet weather, even if the temperature is precisely the same. Moist air is a better conductor of heat than dry, because water, though of the same temperature of air, will carry it off more rapidly. The effect produced by fanning will serve to elucidate that occasioned by the wind; the latter is more active and more extensive in its nature, it carries the warm air to, and brings the cold from, a greater distance.

When these two causes unite, we may easily comprehend why the heat from animals should be carried off more quickly, and they experience a greater sense of cold, than in still dry weather. This also enables us to account for the peculiar disagreeable effect moist windy weather has on our system in the colder seasons of the year. The constant production and succession of heat in the animal, and its tendency to pass off by the surface, and this being influenced by collateral circumstances, prevents the animal heat from acquiring the temperature of the air, and from becoming like that, stationary, varying only with the real changes that take place in the atmosphere. The sense of cold felt by us must be owing to the constant escape of heat, and the degree of cold to this escape, and to the celerity with which the air is enabled to carry off the warm atmosphere surrounding us.

That the temperature of the system can be augmented and supported by artificial means, is an unquestionable truth; and that an expenditure of it may also be increased is no less true. Thus, "when heat is accumulated in the system, either by fever, by strong exercise, or by the scorching heat of the sun, nature constantly cries aloud for acids and a cooling diet.

Acids, when taken into the stomach, always check and restrain the generation of heat, or, in other words, when the system is saturated with oxygen only, less oxygen air (oxygen and caloric) is imbibed by the blood in the lungs, and consequently less heat will be evolved in the body. It is upon these principles," says the Rev. Mr. Townsend, " that the reapers in the south of Spain covet their guzpacho, composed of bread, oil, and vinegar: the two first articles for nutriment, and the latter to moderate their vital heat. On the same principle, obedient to the voice of nature, during the sultry heats of summer, we equally desire our lettuce, oil, and vinegar."—*Medical Extracts.*

Oil not only serves for nourishment, but has also a tendency to relax the solids, as is experienced by those who make much use of it: this is very obvious in parturition. On this principle it may promote perspiration, and hence reduce the temperature of the system*. If my position, " that heat always tends to support an equilibrium," should be objected to, because we can live in a temperature warmer than that of the system, as is proved by those employed in glass-houses, founderies, &c. I would reply, although this is a fact, yet I am not certain that any person, in such a situation, will continue to enjoy good health; and there is no doubt but those thus situated are shorter lived than those in more healthy situations; and, while in this increased temperature, there is a continual increase and diminution of excitement, and the temperature of the body is equalized and moderated by perspiration. Some substances have a greater capacity to receive and retain

* On conversing with an intelligent gentleman on this occasion, he told me he believed I was correct, although the idea was new to him. He observed, that last summer he had eaten much oil, but never before, and that he experienced less inconvenience from heat than formerly.

heat than others; some to receive it, others to retain it, and others to impart it. Spirits boil at 176° of Farenheit; water at 212°; at 75° the animal system is pleasant, and there is emitted an aëriform exhalation; and at a higher degree an aqueous one. Although there is a mutual affinity between animal heat and the temperature of the atmosphere; yet I do not think it necessary to health, that that difference which generally exists, should always be precisely the same; or that either an increased or diminished atmosphere of ten degrees should always produce a similar effect in the animal heat. To say that heat always tends to an equilibrium, is not saying that it always supports one; nor is asserting that there is a tendency to support one, as established by the God of nature, saying that this is always done: my position is, that, when a departure from this general rule takes place, there is a tendency to re-establish it. We know that collateral circumstances have great influence on animal heat; also, that there is in different bodies or substances different capacity for receiving, retaining, or imparting it; and further, that, when the animal system becomes oppressively hot, that it is relieved by perspiration, &c. May not this also prevent an accumulation of heat beyond a certain degree, or, if it is accumulated beyond that degree, will not immediate death be the consequence? When a violent stimulus produces great increased excitement, unless this can soon be reduced, an apoplexy, &c. is the consequence. Heat is an active stimulus, and produces increased excitement; and, when prevented from escaping by its natural outlets, no doubt occasions apoplexy, &c. I cannot believe, because a person who is in a temperature of 120°, and his own is not that, that heat is not a stimulus; nor can I believe, because a person is in one of but 12°, and he has a temperature of 90° or more, that cold is not a sedative. Where there is an increased temperature of the atmosphere, there is

an increased animal heat; if it is so great as to be disagreeable, we assist nature in endeavouring to reduce it. When the temperature is considerably diminished, we have access to artificial means to support it; if these fail, and it continues to diminish, a torpor, sleepiness, and death is the consequence.

The fate of sir Joseph Banks, Dr. Solander, and others, in their botanical excursion on the heights at *Terra del Fuego*, proves the effect of cold on the system. Dr. Solander had more than once crossed the mountains which divide Sweden from Norway, and knew well the effect of its extreme cold. He therefore conjured the company to always keep in motion: "whoever sits down," says he, "will sleep, and whoever sleeps will wake no more." The doctor was, however, overcome with cold, and wanted to lie down; sir Joseph remonstrated, but down he lay. His friend, with great difficulty, kept him from sleep, and, partly by persuasion, and partly by force, he was induced to go forward. Soon, however, he declared he would go no further; sir Joseph again expostulated; the doctor said he would go on, but must first take some sleep, although he had told them "to sleep was to perish." In a few minutes he fell into a profound sleep, and, after five minutes, sir Joseph happily succeeding in waking him, he had almost lost the use of his limbs, and the muscles were so shrunk, that his shoes fell from off his feet. Every attempt to relieve an unfortunate black man, who was similarly affected, proved unsuccessful.

"The ten thousand Greeks, in their memorable retreat in passing through Armenia, were exposed," says Xenophon, "to a contest still more dangerous than the enemy, in which neither skill nor valour could avail. The snow fell in such quantities through the night, as to completely cover the men

with their arms. Their bodies, when freed from the snow, were benumbed and parched with the piercing coldness of the north wind. Many slaves and sumpter horses perished, with about thirty soldiers. It was observed that those died who did not use sufficient exercise." But why go so far from home for facts to prove this melancholy influence of cold, when every winter furnishes us with so many cases in our own country? Can these effects be attributed to any stimulating power which cold possesses?

Cold, under some circumstances, exhilarates; but how? not by any stimulating property it possesses, but by arresting too great an expenditure of excitement. It may be asked, can cold both cause and arrest an expenditure of excitement? Under the last head, I observed a wish to wave every consideration of indirect causes and effects; however, as I have been inadvertently drawn into this remark, I deem it expedient to take some further notice of it.

We know that some medicines have both a direct and an indirect effect. Opium is directly a stimulus, and indirectly a sedative; so is ether, spirits, &c. and so is heat. After being in a temperature of 25°, if we go into one of 50°, we shall be exhilarated, and experience those pleasant sensations occasioned by an agreeable stimulus operating on an increased excitability. But let this heat be augmented so as to produce an excessive perspiration, and let this be supported for some hours, and we shall be much exhausted; but, on going into a cooler temperature, will be refreshed; the stimulus will be less, and there will not be so great an expenditure of excitement; there will also be an accumulation of excitability, and the subsequent necessary stimulants will have a more healthy operation on the system: such is the effect as regards summer.

After several months hot weather, we are pleased to feel a cool evening: it arrests excessive perspiration, and enables the relaxed muscles to regain their healthy tone; not by stimulating, but by preventing too great an expenditure of excitement, &c.

Independently of the effects experienced on the body through the influence of animal heat, there are also other causes which materially affect the system, even when in health. When the temperature of the atmosphere, &c. are precisely the same, both our mental and corporeal system are materially affected. There is a very close connection between the two; and when one is in a morbid state, the other frequently participates. There has been supposed a very great analogy between the nervous and electric fluid; the latter is greatly under the influence of the weather, and we cannot doubt but the former is as much so. The nerves are the seat of all sensation, and all impressions are made on and conveyed through and by them. If the nerves are the vehicles of all sensation, and they, like electricity, influenced by the weather, ought we not to attribute in some degree our peculiar feelings to their direct influence, and to the indirect influence of the weather, &c. through them?

I have dwelt longer on this part of my subject than was intended; but its importance in a medical view, and a wish to elucidate so ambiguous a part of our science, will, I hope, guard me from censure. I shall now proceed to explain some of the phenomena peculiar to the practice of medicine, on the principles which are here held forth.

1st. In syncope, cold is supposed very beneficial.

What is the cause of syncope? Is it ever owing to cold, or are the effects of excessive cold and syncope similar? Is not this disease more frequently occasioned by heat than any other cause? When heat is the cause of it, our theory is immediately comprehended. But it is also produced by severe injuries, loss of blood, impure air, &c. What is the condition of the general system immediately preceding it from these causes? Let syncope proceed from what cause soever it may, is there not, directly preceding it, a great exertion in the general system? does not the universal and copious sweat pronounce an increased heat? It is immaterial what this heat is owing to, whether re-action or any other cause: when cold is applied, it immediately tends to equalize the heat, to accumulate the excitability, and prepare the system for other remedies. It also, by causing an expenditure of the superabundant heat which relaxes the cutaneous vessels, enables them to contract and perform their ordinary function. A similar effect is produced in the lungs; but are we to depend solely on cold in syncope? If the cause is not removed, will the effect cease? When it is in consequence of heat, cold removes the cause. If it is from injuries, loss of blood, contaminated air, &c. will it cease while the cause continues? and when the cause is removed, do we not often find it necessary to have access to other remedies than cold? such as ether, volatile salts, &c. These do not operate as cold; it, diminishes the excitement, and causes an accumulation of excitability, and they, are stimulants; and stimulants act with increased energy on an accumulated excitability. When the patient begins to revive he calls for volatiles, &c. to stimulate the nervous system, and their effect is conveyed through this channel to the general system. He also begs the pure air to be admitted, and to be washed with cold water. Why? because they cool and make him feel pleasant. Do stimulants directly produce this effect?

2d. Cold is salutary in asphyxia.

The observations under the last head, respecting the operation of cold, are equally applicable here. When asphyxia is occasioned by *carbonic acid gas*, and cold water is applied, has it not a tendency to correct the atmosphere by absorbing some of the noxious gas* ?

The explanations given under this proposition enable us more satisfactorily to account for some of the phenomena, partially explained under the first, relative to the use of cold in fevers, &c. and, were it not a work of supererogation, we might now proceed to notice its effects in typhus fever with preternatural heat of the skin ; also in the typhoid state of fever, cholera infantum, &c. Its effect in the synocha and synochus states of fever is generally comprehended.

PROPOSITION III.

There is inherent in the muscles and cellular membrane, a power which produces contraction, when the excitement that expands them is removed. Haller says, " the muscles possess a contractile power, by which they both resist extention, and, when the extending power is taken away, acquire their former shortness ; nor does this power ever cease endeavouring to

* Water receives, when agitated, &c. its own volume of carbonic acid gas, and it is very abundantly absorbed by a solution of pure potash. These facts dictate some important truths, advisable to be attended to, both in prescribing for persons affected with this gas, and also in correcting it. Lime has an extremely strong affinity for carbonic acid, which enables it to take this acid from other substances. Lime, when exposed to the atmosphere, first acquires moisture, and then carbonic acid. Hence, in part, the discolouration of white-washed walls, &c.

bring the elementary particles into the closest contact the mechanism of the part can admit. After death, even for many days, it does the same; so that fibres of a divided muscle contract towards each extremity, and leave a wide gap in the middle; also arteries, when divided, contract themselves in length." This force he calls dead, because it continues after death, and is so far different from those powers of life. In the living animal they are more lively than in the dead one. Again, the cellular fibres are perpetually endeavouring to shorten themselves, and always tend to their own contraction. Hence, when the skin or any other membrane is extended, as soon as the cause of extention is taken off, it returns, by a gentle effort, to its former shortness. It is the nature of this power to act continually by a gentle but uninterrupted effort." *Haller's Physiology.*

Blumenbach, in speaking of vital energies, says, " the first and most universal species of those vital energies, and which may, indeed, be regarded as an inferior degree of the others, or rather as the threshold leading to them, is simple contractility; i. e. a propensity in a part to contract itself." Contractility he supposes peculiar to the cellular membrane, and that it pervades almost the whole body. That this membrane possesses contractility, he thinks obvious, from the construction of the *dartos tunic*, the spasm of the skin, or from the peritoneum, which alone appears sometimes to incarcerate and strangle the intestines in cases of hernia. He supposes this energy so extensive as to even include the bones. See his *Physiology.*

Although I have great veneration for the charcter of Blumenbach, and put much confidence in his judgment, yet I believe the muscles as well as cellular membrane possess this

energy; however, either of the doctrines are sufficient to explain my principle.

That there is inherent in the system such a power, has been proved by many experiments, and is daily proved by incontestible facts.

Dissect a muscle from the body, and stretch it by manual force or any other means, and so soon as the cause of extension is removed, the muscle will contract. In antagonist muscles, if one of them becomes injured, so as to lose all power of contraction, the other contracts the limb. After death, the muscles or cellular membrane continue to contract until the body, which was quite relaxed, becomes very rigid and smaller. Were it requisite, instances of this kind could be adduced *ad infinitum*.

There is inherent in the cellular membrane and muscles a power which produces contraction when the excitement that expands them is removed. Here orignate two questions of some magnitude.

1st. What is it that expands the muscles?

2d. How is this expanding power removed?

First. If the muscles are not directly and entirely expanded by heat, it is a very powerful agent in this process; nor can the expansion be continued without it. How does heat produce this expansion?

It is one of the established laws of physics, that heat expands every body or substance into which it is admitted; whether aëriform, fluid, or solid. The blood is considerably rarefied by heat, and is consequently the easier propelled into and through the minute vessels. That the blood-vessels are expanded by heat may be easily proved. Deprive any muscle in the body of heat, and the vessels which are distributed through it will soon become impervious; and, if the blood does not continue to circulate through any muscle, in an ordinary manner, it will soon be deprived of part of its heat. In old age, many of the cutaneous vessels, if not entirely impervious, are so much so, as to perform much less completely their function, than at an earlier period in life; the general circulation is also slower. Hence, old persons possess less animal heat than the middle aged, and these than the younger. "Now king David was old and stricken in years; and they covered him with clothes, but he gat no heat." The next remedy prescribed was a young virgin, who was to stand before the king and cheer him, and to lie in his bosom that he might get heat.—I Kings, Chap. i. That persons who have much animal heat impart it to those who have less, is a fact generally known; and that young persons possess it in a greater degree than old, is not less so. The modern theory of animal heat is so well known, not only as respects its generation, but also its evolution through the whole system, that to dilate on this part of our subject would be trespassing on the reader,

If heat has this effect in the expansion of the muscles, the answer to the second query, How is the cause of that expansion removed? will be, by removing or expending the heat. This explains more completely our doctrine; and that this is the fact I have not a doubt. Heat has always a tendency to support an equilibrium; hence it is very obvious, that, when

cold is applied to, or comes in contact with the animal system, it must occasion an expenditure of the former. The muscles and cellular membrane always contract when the excitement that expands them is removed. Heat expands every thing into which it is admitted, consequently expands the muscles, &c. hence, when the heat which expands them is expended, a contraction must ensue.

In the winter, when the heat of the system is expended by cold, there is a paucity of blood in the cutaneous vessels; hence the cause of cutis anserina. The shivering which takes place in cold weather is not owing to any stimulating property of cold, but from a diminution of excitement, and from the contraction of the skin, &c. in consequence of this diminution. If this "muscular action," as it has lately been called, were owing to the application of a stimulus, and this stimulus were cold, the action would be increased in proportion to the increase of cold, which is not the case; but if, according to our position, there is in the cellular membrane and muscles an energy which produces contraction when the expanding power is removed, and this expanding power is heat, then this sensation can be satisfactorily accounted for. It has already been proved, that the animal heat is expended much more rapidly when the atmosphere is damp and windy, than in one which is dry and cold, even if in the former the weather is much the warmest. Hence I conceive it obvious, that this action is not in consequence of any stimulus, but owing to an expenditure or diminution of excitement, and a contraction of the skin, &c. in consequence of this diminution; and that this may be occasioned by cold, but more rapidly by a cold wet air than a dry one, even should the former be several degrees the warmest. This effect is not peculiar to the contraction of those parts which come in contact with cold; for frequently

when the bladder has been much distended, and after its contents have been evacuated, as the sphincter vesicæ contracts, there is felt a general tremor, similar to that experienced by cold. But it is not owing to cold; if it were, it would not be confined to the moment of this muscle's contraction. As the cellular membrane as well as the muscles possess contractility, the constriction of the dartos, &c. can be satisfactorily accounted for, without having access to the aid of the supposed stimulating property of cold.

I have not deemed it necessary to institute any experiments on the present occasion. As to matter of fact, there is no subject with which we are better acquainted than that of cold. But we have been at a loss how to account for its operation on philosophical principles. The many experiments that have been made by various persons do, I conceive, prove the correctness of my opinion. Those lately instituted by Woodhouse, Klapp, and others, are very forcible; and they who have considered cold a stimulus, have fallen into some egregious errors.

Currie's Treatise on Cold Affusion, &c. is certainly an invaluable work, but his principles are erroneous, and, in some cases where he was influenced by them, he committed errors of an alarming nature.

In 1792, he, in company with Mr. Hoffman, an ingenious Prussian surgeon, visited a patient on whom they were going to use the cold affusion. When they went to see him he was in the cold stage of a fever. "He was taken out of his bed shivering, his pulse small and frequent, his extremities shrunk and cold. In this state cold brine was dashed over him as usual, but not with the usual happy effects; his breathing was

for some minutes almost suspended; his pulse, at the wrists, was not to be felt; the pulsations of the heart were feeble and fluttering; a deadly coldness spread over the surface; and when respiration returned, it was short, irregular, and laborious. After the use of frictions on the surface, and particularly on the extremities, of a steady warmth, applied some time to the scrobiculus cordis, and of cordials cautiously administered in small quantities, the pulse of the wrist returned; but for some time it was excessively quick and feeble." Currie, from facts of this alarming nature, learned, " that the cold affusion could not be used with safety during the cold stage of the febrile paroxysm; also, that great precaution is necessary in the use of this remedy, and that it ought never to be forgotten, that an application of cold, which is safe in the violence of a fever, is not safe when the fever is removed; and that injuries have sometimes occurred from continuing the cold affusion in the period of convalescence; and further, that it is not safe after the sweating stage of fever has continued some time, and the body is passing through that cooling process." See his treatise.

After this view of the subject, I conceive I may, with propriety, draw the following inferences:

1st. Every thing which directly reduces the excitement is a sedative:

Cold directly reduces the excitement;

Therefore cold must be a sedative.

2d. Caloric or matter of heat has always a tendency to support an equilibrium:

Caloric and animal heat are the same;

Therefore must operate on, and be influenced by the same principles.

3d. There is inherent in the cellular membrane and muscles an energy which produces contraction, when the excitement that expands them is removed:

Heat is materially interested in expanding them, and it is not accomplished without;

Therefore they must contract when the animal heat is expended, or when there is a diminished excitement.

Two Anomalous Cases of the Venereal Disease. Communicated to the EDITOR.

Monmouth County, New Jersey, November 10, 1808.

SIR,

THE following two cases may, perhaps, not be entirely uninteresting to some medical gentlemen. If you deem them worthy of a place in your valuable Museum, I shall feel gratified at their obtaining publicity through so respectable a channel.

Yours, with respect, &c.

W. G. R.

Dr. John R. Coxe.

In 1801, I was attached to a United States' ship, of the squadron destined for the Mediterranean station. In our passage across the Atlantic, and upwards of five weeks after our men had ceased to communicate with the American sea-ports, William Cogan, seaman, complained to me of a swelling in his left groin. As the venereal had, for some weeks previous, been a very common disease among a number of the ship's crew, I suspected his complaint to be of similar origin; but, on examining, I could discover no topical affection, nor any other symptom of disease than an induration in one of the glands, without discolouration. He said he had not observed any sores on, nor discharge from the penis, at any recent period; but that, about a week before leaving Philadelphia, he had indulged in sexual intercourse with some of the *girls of the town.*

As no visible mode of his receiving the disease could be discovered, I rather viewed his complaint as the offspring of cold, or of some other cause of glandular obstruction, less serious in its effects than the venereal disease. I dismissed him for the time with some slight application, and a direction to return if the complaint became more troublesome.

About twelve days after, he returned. The tumor had then acquired the size of a hen's egg; was hard, of a lively red colour, and painful to the touch, with sharp pains darting through it. I directed an emollient poultice to the parts, to be renewed two or three times each day; enrolled him on the sick list, and returned him off duty. Six days after, the tumor burst, and discharged a matter resembling, in colour and consistence, the matter discharged from an ordinary bubo. Soon after the tumor burst, the tumefaction and quantity of

the discharge abated to a considerable degree, and in three or four days the matter had degenerated into a thin sanies, which wept from the sore, and seemed to prevent the healing or closing up of the same; at the same time the soreness and rigidity of the parts had so far abated, as to enable him to do light duty on board the ship.

For ten days after his return to duty, the ulcer continued to retain much of the last named appearance, and I remained undecided in respect to the most proper method of its treatment. I was aware of the various anomaly in the symptoms of complaints *purely venereal;* and that a primary affection was communicable by absorption alone, without any external ulceration, was an opinion I knew to be admitted by very respectable medical authority, but an opinion I supposed to owe more of its existence to anatomical or physiological inference, than to any well-grounded experience on the subject. I had never met with any case, or well authenticated history of one, that placed the matter beyond doubt.

About the expiration of the last named period, he informed me that he felt a hard lump, with pain, in the right groin. I examined and found two indurations seated in the lymphatic glands of the penis. I marked their daily progress for about eight days, at the end of which time they began to be considerably enlarged, inflamed, painful to the touch, or, on moving the limb, accompanied with a slight acceleration of pulse.

The true character of the disease was now too conspicuous to be mistaken. I directed mercury (principally by inunction) in such quantity, that on the sixth day of its exhibition a ptyalism was perceptible. The tumors remained stationary for several days, then began to diminish in tension and magni-

tude, to be less painful, and finally to disappear by the twentieth day from the first exhibition of the mercury. On the twenty-fourth day the ulcer was entirely healed, and on the twenty-sixth the medicine was discontinued, at which time every symptom of the disease had disappeared. A slight soreness of the mouth (which had been constantly kept up) soon subsided, and the patient was enabled to return to active duty with his accustomed health and spirits.

Four months after his recovery he contracted the venereal disease in one of the Italian sea-ports. An extremely obstinate chancre was the first symptom, which was succeeded by a bubo equally obstinate: the latter was strikingly similar, in every stage of its progress to maturation, to the buboes above mentioned.

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About the 20th of May, 1808, P. F. with visible trepidation of mind, complained to me of what he feared to be a venereal infection, contracted about four weeks before in New York. He said he had for several days past felt pain in the urethra, sometimes darting along the perinæum, and in his hips, running thence to the calves of his legs, with obtuse pain in the shoulders, communicating to the arms. No symptom of disease was apparent on or about the genitals, nor had he discerned any from the time he supposed the complaint to have been contracted. Observing the state of his mind, and no other evidence of disease discoverable, I, in my mind, referred his complaint to that variety of the disease which our worthy professor Dr. Rush has sometimes, in his lectures, denominated *noddle pox*, dismissed him with a placebo, and assurances that he would be relieved in a week. At the expiration

of that time he returned, said all his pains were more severe, with occasional titillation at the extremity of the urethra. I now thought that an incipient calculus might possibly occasion the titillation in the urethra, and an alarmed imagination create every other symptom. I gave some gentle laxatives, directed demulcent drinks, and dismissed him for another week. Before this week expired he returned, said he had repeatedly discharged blood from the urethra since he last saw me; and that, in addition to the above symptoms, he had occasionally giddiness in his head, pains shooting across his breast, hurried respiration on moderate exercise, broken slumbers, and, in short, a feverish restlessness throughout the whole frame, without being able to point out any particular seat of disease. I caused him to void urine in a vessel containing water, when two or three clots of blood, the size of a pea each, were discharged with the urine; I searched with an elastic catheter, but could find no part of the urethra preternaturally tender. There was sometimes a slight obstruction to the urine on his first attempting to void it; but the inclination to void it was not more frequent than is common in a healthy subject. There was no tenesmus, priapism, nor enlargement of the prostate gland; his pulse was small, with a little tension in it.

During a long and extensive acquaintance with the venereal disease, practitioners must occasionally meet it under various disguised forms; but it is rarely, I believe, that the most insidious cases may not be detected either by their previous history, a topical affection, some antecedent similar case, or the presence of desultory symptoms, among which experience can discover the venereal as an ingredient. As to the case in question, I now strongly suspected its venereal origin; but, to adapt a successful mode of treatment thereto, was a more embarrassing task: it seemed like attacking an enemy in am-

bush, who assailed you from all quarters, without discovering in what direction his principal strength lay. Suffice it to say, that the symptoms abated much within three weeks, under the copious use of decoct. sarsaparillæ, emulsions of pearl barley, a light soup diet, aperients, a daily moderate use of tinct. g. guaiac. and warm injections into the urethra, composed of sweet milk, and milk and almond oil, blended with a few drops of tinct. opii added to each injection. From the end of the third week, the sarsaparilla and barley emulsions only were used, with a few of Dover's powders daily. At the end of the fifth week, every vestige of disease had vanished, and invigorating diet was recommended, which soon recruited the patient.

To the young and less experienced physician, the former of the above cases presents two considerations of much practical utility. The first is, that whereas the venereal poison may be communicated without any visible marks of inoculation, the absence of the signs by which we generally judge of its presence ought in no wise to prevent the use of proper remedies, when the affection of the system sufficiently points out the nature of the disease. Secondly, as the seeds of the disease may lurk for a length of time in the system, without affording any evidence of their existence there, this fact ought to influence the practice in the treatment of recent local affections: during the cure of these, to preclude every possibility of the lodgement of the virus in the system, by a well-directed course of such medicines as experience declares to be the most proper for that purpose, must be acknowledged a prudent and judicious practice.

The second case offers a question to the etiologist, viz. What was the particular modus operandi of the virus in the

system? Did a lodgement of the poison in any part of the urethra affect the more distant parts of the system by sympathy, or did it enter the circulation through the absorbents of the urethra or glans penis, thence affecting the solids in a manner peculiar to itself, analogous to mercury in a salivation, or is there a still more rational mode of explaining its operation? Without attempting to decide this point, I will remark, that if the idea of absorption is admitted, it will furnish an argument in favour of the distinctive natures of gonorrhœa and lues venerea, as no symptom of lues appeared during the whole complaint.

Case of Monstrous Birth. In a Letter to the EDITOR, *from Dr.* STRYKER.

Somerset, New Jersey, February 28th, 1809.

SIR,

IF you conceive the following case merits publication, I shall be gratified to find it obtain a place in your Medical Museum.

I am, sir, with esteem,
Your most obedient servant,
P. J. STRYKER.

Dr. J. R. Coxe.

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On the —— day of ———, I was requested to visit Mrs. F. in labour. Her pains regular and of sufficient force, the os tincæ handsomely dilated, and much water protruding the

membranes; in the intervals between the pains endeavoured to ascertain what part of the child presented; but could only satisfy myself that the presentation was not natural. I determined to introduce my hand, break the membranes, take advantage of the abundance of water to examine more accurately the part presenting, and then act agreeably to the exigencies of the case. The presenting part was, in fact, what must be termed the head, and the child was delivered with much facility: it proved an uncommonly large female, and, at the instant after delivery, lively and strong; insomuch that, before I had time to reflect, feared it would survive. The whole of that part of the cranium or brain case, with its usual contents, which is naturally covered with hairy scalp, was absolutely wanting, and the foramen magnum occipitis covered with a bloody excrescence; its face and every other part perfectly formed. It lived, perhaps, three or four minutes, rolling its large full eyes about most part of the time. This was Mrs. F.'s fourth child; all daughters.

Mr. F. was a gentleman of uncommon vivacity, and frequently, during Mrs. F.'s pregnancy, when he came in her room and found no third person present, would walk up to her, seize her cranium with his fingers extended, and observe humorously, "that if she presented him with another daughter instead of a son, thus he would pinch it in the head."

Mrs. F. was a nervous lady, and this farce, frequently repeated, at length gave her so much pain or (rather) horror, that she informed Mr. F. of her sensations, and intreated him to desist.

This account I received from both Mr. and Mrs. F. the latter of whom does not even at this day know that there was any deficiency in the formation of the child.

Query. Could the conduct of Mr. F. (as above related) have had any share in producing the imperfection of this child?

A Case of Sudden Death, with a Singular Appearance of the Lungs on Dissection. *By* W. G. R.

Monmouth County (*N. J.*), *April 5th*, 1809.

ON the second ult. I was requested to visit David Crawford in much haste. Unavoidable circumstances protracted my arrival at the place for nearly three hours after his attack. I found him dead, and the following was the account given me of his case. He had sat down to supper in a very cheerful mood, and, on taking a mouthful of food, began to laugh heartily, though not immoderately, when all of a sudden his voice stopped. A gentleman seated at table with him, observing him make several efforts to breathe, asked him if he was *choaked;* but receiving no answer, rose to assist him. By the help of a servant the gentleman raised him from the chair, when he sunk almost lifeless from their hands, and was extended on the carpet: here some half-fetched respirations, frothing of the mouth, and convulsive heavings of the chest, marked his exit in little more than ten minutes after his first attack.

Mr. Crawford had reached the sixty-second year of life, and enjoyed a greater share of robust health than is common at that period; his conformation of body was apparently

faultless, and his appearance, when I saw him, indicated sleep rather than death.

The circumstances attending this disastrous occurrence, inclined me to consider it a case of strangulation by the inhalement of some particles of the food into the trachea, or bronchial ramifications; and here I would ask, does the faculty of medicine possess any generally successful mode of treating accidents of this kind? I must acknowledge myself unacquainted with any that are either practised or recommended; and I have known a physician to remain an impotent and almost idle spectator, in more than one or two distressful cases.

I am of opinion that the danger or risk of life to the patient, is in proportion to the stage of inspiration at which the offending material is taken into the lungs. If at or near the end of an inspiration, any extraneous body, as a crumb of bread or drop of water, finds its way through the glottis, the convulsive efforts in the respiratory muscles that immediately follow, expel the air with vehemence, and the body is extruded by the current thereof. If, on the contrary, the lungs are nearly empty at the time of receiving the accident, the efforts in the muscles are equally powerful; but, as there is no expelling power within, the cause necessarily remains; and on every endeavour to inspire air (which is apt to be attempted with too much hurry and force) these convulsions are accelerated or renewed, until at length the system sinks for the want of those principles* of atmospheric air so essential to its

* I say principles, because I have for some years past indulged an opinion, that something more than merely supplying the system with oxygen, or freeing it of carbon, is the object of respiration. Some experiments relative to this subject are going forward, which, when sufficiently matured to meet the public eye, I will offer to the Museum.

continuance in a state of life and motion. Several plausible considerations incline me to believe, that not only in occurences of the above kind, but in convulsive laughter, in drowning, and respiring some of the gases, the system incurs more danger by the sudden and total privation of atmospheric air, than in any paralysis consequent to convulsions or other violence done to the system by the vehement action of the heart and arteries.

The first and perhaps only indication on the introduction of a foreign body into the larynx, is to remove the offending cause. How is this to be effected? Tracheotomy, unless indicated by the size, position, or qualities of the body, must be deemed a very unwarrantable mode; and the practice of giving emetics, which I have heard some physicians suggest, is too absurd to require refutation. Inflating the lungs by means of a pipe introduced into the glottis, or even by blowing through the patient's nostrils, might afford some advantage. I know objections may be offered to this practice, but, in my opinion, they are too trifling to forbid its application when the life of an individual is depending, and every other prospect of succour is cut off; it may be employed with equal propriety and more ease, after the system has ceased to re-act. Indeed, I think that attempts at resuscitation, in cases of long suspended animation from this cause, offer as fair a prospect of success, as in like cases of the same standing from any other cause. Should attempts to resuscitate be objected to, under the supposition that they would only subject the patient to the misery of dying twice, by reason of the offending cause, when it remains, reiterating its effects upon the respiratory muscles, as soon as the principles of life return: to these objections I would reply, that, as far as my experience goes, the human

body, on recovering the spirit of animation after a long suspension thereof, does not immediately require its wonted aptitude to motion, or susceptibility to impressions on any of its parts, but is less excitable for an uncertain length of time; in some instances for many days. Under these circumstances, any extraneous matter lodged in the larynx, might be thrown out by moderate coughing or other natural efforts of the organs subservient to respiration; and should such matter be impacted into any of the terminal ramifications of the bronchia (which might easily happen during the resuscitative process), it would either be dissolved by the natural moisture of the parts, or excite suppuration; and, in either case, would be thrown off by expectoration.

These ideas accord with experience, and, I think, with the known laws of the animal economy. I can adduce a living instance (and there are many more no doubt) in which the fragments of a cartridge paper, lodged in the lungs by a gun-shot wound, was discharged many months after the injury had been received, by an expectoration of purulent matter; and another, in which cheese-like concretions were expelled from the lungs after the patient had laboured under a diseased and very weak state of those organs for a period of two or three years. I lately witnessed an attack of strangulation in a lady of my acquaintance. She suffered no person to touch her, but aimed solely at collecting, by little and little, a full inspiration, which, when accomplished, she forcibly threw off, or expended by, apparently, a voluntary effort. After a second inspiration acquired and disposed of in this way, she was much less distressed, and soon became perfectly easy and tranquil. She told me that she had often suffered similar at-

tacks, and in in each of them aimed at the same means of obtaining relief*.

To return to the case of Mr. C. Attempts to resuscitate the body were not deemed advisable, as well on account of our uncertainty in respect to the precise cause of the death, as of the very little prospect of success which attempts of this kind afford in cases where life has been suspended for so great a length of time. I opened the body, with a view to obtain better information as to the cause of the occurrence, than opinion or conjecture alone could afford me. The circumstances admitted of but a hasty and imperfect search, in which I was not able to discover any mark of recent disease, save a slight effusion of blood into the cellular substance of the lungs. The lungs, which were the only organs particularly inspected, adhered, throughout their whole anterior and lateral surface, as firmly to the walls of the thorax, as the skin does to the muscles in a healthy state of the body, by means of the cellular membrane. Traces of the mediastinum were with some difficulty recognized. This partition had acquired a texture nearly resembling the substance of the lungs; and, I have no doubt, performed, in some degree, the function thereof. The right and left lung, with their respective lobes, had ce-

* Some years ago, in making a trifling experiment with crude mercury, I carelessly threw a quantity of the metal into my mouth, and felt a portion of it rush through the glottis; a strong inclination to cough was the immediate consequence: I could, however, nearly restrain the convulsive efforts of the muscles. I quickly inverted the natural position of my body, and requested a gentleman present to jolt me up and down. Two or three globules of mercury, each the size of a pigeon-shot, directly fell into my mouth; and, on assisting the jolting by forcible expirations, several smaller globules followed, making in all about a scruple. I felt no inconvenience afterwards from the accident.

mented as it were into a single mass of lung; the pericardium had either adhered to the concave surface of this mass, and been assimilated to the nature and appearance of the same, or had been obliterated by some other process, for there was no such sac or membrane to be found; the heart was imbedded in the inferior posterior portion of this mass, and almost covered by it even in its flaccid state. The diaphragm was free from any adhesions, as was also the great curve of the aorta. I could perceive nothing that resembled tubercles in the substance of the lungs.

Some days after the examination of these organs, I mentioned the singularity in their appearance to an acquaintance of the deceased. He informed me, that, about the close of the American revolutionary war, Mr. C. had laboured under a very advanced stage of pulmonary consumption for a long time; that several of his friends, on visiting him in that state, would take, as they thought, a final leave on parting; that he recovered slowly, but under what particular treatment, or whether he had medical assistance, this gentleman could not recollect.

I am happy in being able to furnish an instance wherein a long series of good health succeeded to a very diseased state of the lungs; and most probably that form of disease termed consumption. Some physicians there are who strenuously adhere to the opinion that consumption is an incurable disease. How a mind keeping pace with the progress which medicine has made since our own time can seriously admit such an opinion, I am unable to conceive; but it is to be hoped that more demonstrative evidence than heretofore, or more correct modes of thinking, will remove from such minds an error unpardonable in a physician, and hostile to the lives and happiness of many individuals of the human family.

Account of the Failure of the Geranium Maculatum, in stopping Hæmorrhage. Communicated by WILLIAM BALDWIN, *M. D. of Wilmington, in the State of Delaware.*

J. H. a young man of this borough, punctured the radial artery at the wrist, with the sharp corner of a currier's knife, on the 25th of August. The hæmorrhage was profuse. Compression was employed till the night of the 30th, at which time the blood flowed as profusely as ever. The root of the geranium maculatum, recently dried and powdered, mixed with a little cold water, was now plentifully applied, and secured with a compress and roller. Appearances were favourable till the night of the 5th of September, when an alarming hæmorrhage again took place. The geranium was again resorted to, but on the following day the recurrence of a third bleeding rendered the operation indispensible, and the artery was secured with a ligature.

Whether the root, in a dried state, is less powerfully astringent than when fresh, as employed in the cases reported by *Dr. Mease*, in the third volume of the *Museum*, it is of importance to determine. To the taste it appears equally astringent, and the form of powder would seem to be better adapted to act with advantage, than the root simply bruised.